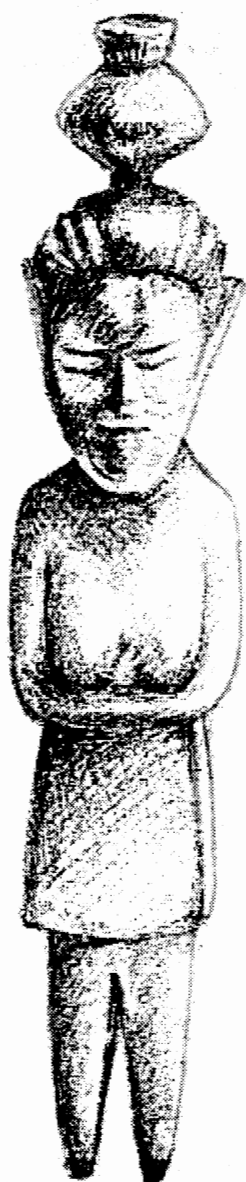


MOTHER TONGUE

NEWSLETTER OF
THE ASSOCIATION
FOR THE STUDY OF
LANGUAGE IN
PREHISTORY



Issue 21, January 1994

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AIM & SCOPE

The Association for the Study of Language in Prehistory (ASLIP) is a nonprofit organization, incorporated under the laws of the Commonwealth of Massachusetts. Its purpose is to encourage and support the study of language in prehistory in all fields and by all means, including research on the early evolution of human language, supporting conferences, setting up a databank, and publishing a newsletter and/or journal to report these activities.

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PROGRESS REPORT FOR PROJECT ENTITLED "A STUDY OF THE GENETIC COMPOSITION OF ANCIENT DESICCATED CORPSES FROM XINJIANG (SINKIANG), CHINA"

VICTOR H. MAIR
University of Pennsylvania

INTRODUCTION

Planning for this research project began in the fall of 1991. I was greatly encouraged when my initial inquiries to the Xinjiang Institute of Archeology met with positive responses. The next year (1992) was taken up with the writing of grant applications and a protracted period of correspondence with various academic institutions and government offices in China. Formal invitations from the Xinjiang Institute of Archeology and the Xinjiang Academy of Sciences arrived about two months before I was scheduled to be in China. This was just in the nick of time for me to obtain the necessary visas and to reserve suitable international and domestic air transportation.

BEIJING (PEKING)

When I arrived in Beijing on the evening of June 18, I was very happy to meet Dr. Paolo Francalacci (my anthropological geneticist colleague from the University of Sassari in Italy) at the airport. He had arrived the same day around noon from Rome via Moscow. After two years of planning and applications, everything was beginning to fall in place.

We went straight to the Shao Yuan Guest House of Peking University and checked in late that night. While in Beijing, I met a number of graduate students from the University of Pennsylvania and a large number of other students who wished to apply to our graduate program. I also held discussions with about a dozen professors from various departments (History, Oriental Languages, etc.) at Peking University as well as from various institutes of the Chinese Academy of Social Sciences and the Chinese Academy of Sciences. In addition, I took advantage of the opportunity to renew my extensive contacts with many applied linguists and language reformers. Most important for my current research project, however, were my meeting with Professor WU Rukang, distinguished professor at the Institute of Vertebrate Paleontology and Paleoanthropology, and Professor DU Ruofu of the Institute of Genetics. Since this was the first time I had met Dr. Francalacci in person, I spent a lot of time detailing his role in the project, and he patiently explained the principles of PCR (Polymerase Chain Reaction) amplification of mitochondrial DNA, the main technique we would be using in our analysis.

ÜRÜMQI

On June 22, we flew from Beijing to Ürümqi in the heart of Central Asia, where we were met at the airport by WANG Binghua, Director of the Xinjiang Institute of Archeology. The first day was spent in getting acquainted with the staff of the Institute and in examining their premises, including an impressive collection of artifacts, skeletons, and corpses kept in two display rooms and several storage areas.

June 23 and June 24 were occupied in intense, serious negotiations which would determine whether or not we would actually be able to take tissue samples on this trip. French and Japanese teams had earlier tried to win similar concessions but had failed. The main concerns of the Chinese were that their geneticists and archeologists would be directly involved in all phases of the work, that they would be listed as joint authors of any significant publications emanating from the project, and that technology transfer would occur in the form of training in the new, advanced analytical techniques we would be using. The Chinese side was also eager to have some of their scholars go abroad for brief visits, to raise money from American foundations to support their work, and to seek assistance in building a special museum for the preservation of the many corpses that have been excavated and will continue to be excavated in Xinjiang. Naturally, taking tissue samples was a highly sensitive matter and had to be cleared both with the Xinjiang Autonomous Region Cultural Affairs Office (Wenhua Ting) and the national Bureau of Cultural Relics (Wenwu Ju). To obtain the permission of the former, deliberations were carried out locally, but to obtain the permission of the latter, in addition to faxes and telephone calls, we had to send the Vice Director of the Institute (Idris) to Beijing on the spur of the moment. In the end, after two days of grueling and exhausting discussions, we did receive the necessary authorizations. The chief reason, I believe, is that everyone who took part in the negotiations realized the beneficial implications which this project holds for the enhancement of our knowledge of Xinjiang and world history.

HAMI

We drove to Hami (Mongolian Khamil, Uighur Kamul or Qomul) on June 25, arriving in the late afternoon. The first evening was spent in meeting local archeologists and museum personnel. Early the next morning, we drove to Wupu, about an hour and a half away from Hami. This is the most important site for our project.

Approximately one kilometer to the northwest of the oasis village of Wupu, there is a small rise of desert land that lies across a stream called Baiyang Gou ('Poplar Gully'), which separates it from the town. The rise is now known locally in Uighur as Qizilchoqa, which means 'Red Hillock.' An area of about 20 acres (my rough estimate) has been fenced in for protection and is under the jurisdiction of the Xinjiang Institute of Archeology which is responsible for its continuing excavation. A brick workshed has been constructed on the site for storing tools and for archeologists to live in when they are working at Wupu. An old man named Imit, whose house is

nearby, is given a small monthly salary to look after the site and report any trespassers.

Qizilchoqa was discovered by WANG Binghua in 1978 by following the stream in the Baiyang Gou from its source in the Tian Shan ('Celestial Mountains') to the north. It was his hunch, based on long experience as a practicing archeologist in the region, that ancient peoples would have located their settlements along the stream because it provided a relatively reliable source of water. As he followed the stream bed, Wang asked the local inhabitants whether they had come across any old broken bowls, wooden artifacts, and so forth. It turned out that Imit was the first to direct Wang's attention to Qizilchoqa.

During the first year, 29 graves were excavated. In 1986, 82 more graves were excavated and, in 1991, two additional graves were excavated in cooperation with a fairly large team of Japanese archeologists. Judging from the size of the plot and the number of obvious depressions in the surface of the land (which indicate the existence of a grave beneath), there are probably at least another hundred graves remaining to be excavated at Wupu.

The Wupu graves have been securely dated to approximately 1200 BCE by the following means: presence of bronze objects, the style of painted pottery, and five C¹⁴ dates which were all consistent. This puts the site at a key point in the development of Chinese civilization. It is situated at essentially the same moment as the introduction of the chariot and the rise of writing.

Aside from the extremely well-preserved ancient corpses, which I shall discuss in more detail below, a wealth of artifacts was recovered from the graves. These were not luxury goods but simple items for use in daily life (combs, needles, bowls, pots, hooks, bridles, bells, whorls, spindles, bread, etc.). Among those that struck me most powerfully was a part of a wheel that I spotted protruding from an unexcavated grave. It was similar to another partial wheel that had already been unearthed from one of the other graves earlier and was kept in the Hami Museum. Since these wheels are of a very peculiar construction, comparison of the wheels elsewhere on the Eurasian landmass should reveal much about the transmission of this technology across cultures. This is especially the case since the Indo-European word for 'wheel' itself (< **k^wel-*, compare 'cycle' from the same root) appears in dozens of languages, including colloquial Sinitic languages (compare Mandarin *gulu*). Needless to say, I shall make every effort to identify the sources of this particular technology.

The Qizilchoqa graves are quite simple in their construction. About two meters deep, they are lined with large, unbaked bricks around the sides. The pits are just big enough for the occupant(s) of the grave, who are placed in them on mats supine with knees bent upward. Above the buried individuals is a layer of large, rough-hewn logs that is located about halfway down in the pit. On top of the logs are mats and reeds to prevent the thin layer (about two or three feet) of sandy covering soil from falling down into the burial chamber.

The most spectacular aspect of the Wupu graves are the ancient corpses themselves. Due to the unique combination of climatic conditions in the area, many of the corpses have

been almost perfectly preserved through a process of natural desiccation. The corpses are fully clothed in splendidly colored woolen fabrics, felt and leather boots, and sometimes leather coats. They are clearly of Caucasoid/Europoid extraction (long noses, deepset eyes, blonde/light brown hair, etc.), a vital matter that I shall touch upon again later in this report. The men are fully bearded, and the women have long braided hair.

We spent the entire day at Qizilchoqa with a crew of local archeologists and workers exhuming corpses that had previously been excavated but, after preliminary examination and recovery of important artifacts, had been reinterred for lack of adequate storage facilities in Hami or in Ürümqi. Dr. Francallaci, wearing a face mask and rubber gloves to avoid contamination of the corpses with his own modern (and much more powerful) DNA, used surgical scalpels to remove small samples of tissues from unexposed areas of the bodies (usually the inner thighs or underarms). We also took a few bones (parts of the ribs that were easy to break off), which preserve the DNA perhaps even better than does the muscle tissue and skin.

The samples were placed in collection jars, sealed, and labeled. While Dr. Francalacci was doing his work, I made a photographic and written record of the tissue collection. Altogether, we took double or triple samples from six corpses at Hami.

One of the female mummies from Hami had been transported to the Museum of Natural History in Shanghai shortly after excavation. There it was subjected to extensive examination for its physical characteristics, histology, musculature, protein and adipose composition, hair keratin and trace elements, and blood type. Papers on these subjects have been published in the Museum's *Kaocha yu yanjiu* (*Investigatio et Studium Naturae*), 4 (1984). Unfortunately, the Wupu mummy that had been examined in Shanghai and which is now in the Xinjiang regional museum was treated with a shellac-like substance which had darkened and hardened its skin to such a degree that it no longer retains its natural color and flexibility.

CHERCHEN

Although we did not travel to the town of Cherchen (Qiemo in Mandarin), which lies toward the eastern end of the southern branch of the Silk Road, we were able to see about half a dozen corpses from the very important site of Zaghunluq that lay in the desert nearby. These were kept in the district museum at Korla, which we did visit. While there, we took tissue samples from two of the corpses that were best preserved, a young woman of about 20 years old and a little boy of approximately 2½ years.

I had previously seen three magnificent specimens from Cherchen that were kept in the Museum of the Xinjiang Uighur Autonomous Region in Ürümqi. That was in 1987, just after they had been excavated. These were a man and a woman, both of great height, together with their child (offering exciting possibilities for genetic research). I had also, a year or two later, witnessed a slide show of their exhumation by the excavator, Dulkun Kamberi, who is now studying for his Ph.D.

at Columbia University. An article by Kamberi on the Cherchen corpses will appear in *Sino-Platonic Papers* during 1994.

This group of Cherchen mummies — which date to about 1000 BCE — are startlingly lifelike. It was the uncanny experience of observing these freshly exhumed corpses in 1987 that first prompted me to think about the possibility of studying their genetic affiliations. The sensational discovery of the Austrian “iceman” in 1991 and the knowledge that analysis of 6,000 year old frozen DNA would be carried out, cemented my intention to organize similar studies of the ancient Xinjiang corpses. I should note, parenthetically, that it is only since the late 1980s that the techniques of DNA analysis have been sufficiently refined that they can be applied to the study of ancient corpses. So far, only a few such ancient corpses have been studied (such as those by Dr. Francalacci of Etruscans from 700 BCE and Egyptians from 1000 BCE). This means that our project is on the cutting edge of a very new sub-field of archeology. Xinjiang offers vast potential for the application of these new techniques. At Cherchen alone, estimates for the number of unexcavated graves range from 25 (definitely too low) to 750 (probably too high).

LOULAN

Loulan (ancient Kroraina) lies next to the shores of the dried-up Lop Nor. In 1980, a team led by MU Shunying recovered from the lower reaches of the Tierban River a stunningly beautiful corpse, aged about 40-45, together with an infant. Their date, determined by stratification, C¹⁴, and associated objects, is 2000 BCE. The woman has become known as the “Beauty of Loulan” and is the centerpiece of the ancient corpse display hall in the regional museum at Ürümqi. The woman was recently taken to Japan for exhibition in Tokyo, Fukuoka, and Kyoto, and she now lies peacefully in a wonderful glass case like a Sleeping Beauty waiting for her prince to come awaken her. The catalog of the exhibition was published in 1992 by Asahi Shinbunsha as *Rôran ôkoku to yûkyû no bijo* (*The Kingdom of Loulan and the Eternal Beauty*). Elaborate scientific studies of the corpse were carried out at the Shanghai Museum of Natural History and have been published in *Kaocha yu yanjiu* (*Investigatio et Studium Naturae*), 7 (1987).

Tissue samples of all the desiccated corpses in the display hall of the regional museum at Ürümqi will be taken as part of our project. We have trained a young geneticist (CHEN Jian), who is teaching at Xinjiang University, how to do this, and he will be responsible for the collection of samples from corpses discovered during the course of future excavations as well. Until China acquires the capability to do the PCR tests on ancient DNA locally, it will be necessary to continue to send the samples to Italy or other laboratories in Europe and America.

SUBASHI

As we were driving to and from Hami, we stopped near the site of Subashi, which lies in the gorge of Toyuq, not far from Turpan (Turfan). Here, during March-April, 1992, a team led by LÜ Enguo of the Xinjiang Institute of Archeology excavated an extremely rich Warring States (5th-4th c. BCE) graveyard. Over a dozen corpses were recovered, as well as an enormous amount of artifacts which reveal fascinating details about the customs and material culture of that period (a woman's hat with a long horn-like projection, apparent indications of polygamy, medicine pouches to be hung at the waist, operations with horse-hair sutures, a saddle, composite bows of complicated structure with cases and quivers, etc.). It is surprising that, at this rather late date, all of the individuals we examined were still clearly Caucasoid/Europoid. The corpses and most of the artifacts are kept in an inadequate basement storage room at the Institute of Archeology in Ürümqi that is damp and cramped. There is a desperate need to provide these precious specimens with surroundings that are better designed to ensure their preservation.

TURPAN

We spent a full day in Turpan, but there was not much of importance for our project. Although the corpses from the famous Astana graveyard are extremely well preserved, and we even know the names and other biographical details about some of them, they are much too late (5th century BCE — Tang period) and are clearly Mongoloid.

It is worth mentioning that the United Nations is sponsoring a major effort (financed with a million dollars from Japan) toward the conservation and exploration of the ruined cliff city of Jiaohe (Mongolian Yarkhoto). Evidence of this ongoing work is plentiful, and I expect major discoveries from this Han-Yüan site.

RETURN TO ÜRÜMQI

The last two and a half days of our jam-packed stay in Xinjiang were dedicated mostly to drafting a statement of intention (attached hereto) and various ancillary documents. We also spent some valuable time in the regional museum studying the marvelous collection of artifacts and corpses there. Half a day was required to examine and photograph the corpses and artifacts from Subashi that are kept in the basement of the Institute of Archeology.

Although the final negotiations were delicate and demanding, the good will and positive intentions of the parties involved made it possible to hammer out an agreement that met with the approval of all. We were especially fortunate that XIE Yaohua, the Vice Director of the regional Cultural Affairs Office, was not just a politically-minded bureaucrat, but someone who was deeply aware of the tremendous progress that could be made if genuine international cooperation were the order of the day.

AIMS AND COMMITMENTS

The primary aim of this project remains that of employing the latest techniques of DNA analysis to determine to the best of our ability the genetic affiliations of the ancient inhabitants of Xinjiang. As more and more data become available for both modern and ancient populations around the world, this will be increasingly possible to realize. We cannot, however, rely on DNA analysis alone, but must supplement it with other types of investigations. Among these is the close examination of the fabrics found in the Xinjiang burials. In this regard, preliminary investigations show a remarkable similarity between plaid woolens from Wupu and from Danish burials at roughly the same time, both in terms of weave and pattern. This is not altogether surprising, considering the results of the physical examination of the Wupu corpses, which are clearly Europoid ("Nordic" in the terminology of HAN Kangxin, who has studied the Xinjiang corpses extensively for the past decade). Much more work must be done on just the fabrics alone, such as optical and electron scanning microscopic examination of the fibers, chemical analysis of the dyes, comparison of looms and other weaving technology, etc. There are many other specialties that we will bring to bear in our study of the ancient inhabitants of Xinjiang, including religious rituals (the Hami corpses faced east, the Cherchen were decorated with ocher, the "Beauty of Loulan" was tightly wrapped in a fine woven mat and had a small woven basket placed to the left of her head, and so forth).

In order to carry out this large research scheme, I have committed myself to five years of fund-raising and coordination that will bring up to half-a-dozen Chinese scholars to the United States, will send the same amount of American specialists to Xinjiang, will promote additional excavations, and will culminate in the building of a climate-controlled museum for the study and display of ancient desiccated corpses.

PERSONNEL

The co-principal investigator for this project with Victor Mair is Dr. Luigi Luca Cavalli-Sforza, the eminent professor of genetics from Stanford University who is at the forefront of the interface among the disciplines of archeology, population genetics, and linguistics. Dr. Cavalli-Sforza had already collaborated frequently with Chinese scholars in the study of such topics as the relationship between ethnic groups (as revealed, for example, through surnames) and genetic types. For comparative purposes, Professor DU Ruofu of the Chinese Academy of Sciences Institute of Genetics will examine the DNA of living populations (Uighurs, Tajiks, Kazakhs, etc.) of Xinjiang. Although the ancient peoples of Xinjiang may have died out as an identifiable race, their genes must survive in the chromosomes of the groups who supplanted them. Hence, one frequently encounters individuals in Xinjiang today who have red, brown, or blonde hair, blue eyes, long noses, and so forth. Professor HAN Kangxin will carry on with his own long-term examination of the physical characteristics of the ancient peoples of Xinjiang. Research such as Han's already indicates

that, around the year 1000 BCE, nearly all of the individuals whose remains have been archeologically recovered in Xinjiang are Caucasoid/Europoid; by the 5th-4th century BCE, only about 60% are Caucasoid/Europoid; by the 5th c. CE, more than 60% are Mongoloid; while by the end of the Tang dynasty (906 CE), the indigenous peoples of the Tarim Basin had been almost entirely replaced by Sinitic and Turkic peoples. Dr. Paolo Francalacci will continue to do PCR amplifications for us at the Universities of Pisa and Sassari and will consult when necessary with colleagues in Germany (e.g., Dr. Svante Pääbo) and elsewhere in Europe and America. Irene Good of the University of Pennsylvania, a pathbreaking specialist on textiles and long-distance trade, will study the abundant textiles that have been discovered in the ancient graves of Xinjiang. We hope to obtain the expertise of Dr. Christy Turner of Arizona State University, the world's foremost authority on the science of ancient human teeth. Others may be invited to join the project as we become aware of the contributions they might be able to make.

POTENTIAL SIGNIFICANCE

My working hypothesis is that the original inhabitants of the Tarim Basin during the first and second millennia BCE were the ancestors of the Tocharians. The latter are well attested during the first millennium CE in historical documents, on local wall-paintings that depict them as red-bearded European knights wearing Sassanian dress with long swords at their waists, and through texts written in at least two variants of their own language that have been unearthed and deciphered during the past century. My hypothesis is in accord with the recent views of young Chinese scholars such as LIN Meicun and XU Wenkan, who would tend to identify the Tocharians with the Yuezhi (or Ruzhi), a people whose name can be traced back to Chinese texts dating to the pre-Qin period (i.e., before 221 BCE). W. B. Henning, a renowned Iranist, claimed that the Yuezhi could be equated with the biblical Guti who invaded Mesopotamia from the mountains to the north around 3000 BCE, but (in spite of some archaic, Hittite-like features shared by Tocharian) the vast separation in space and time make such an assertion doubtful. The noted Indian scholar, A. K. Narain, claimed that the Yuezhi/Tocharians were "the first Indo-Europeans." While it is true that Tocharian is the easternmost branch of the Indo-European language family, there are several difficulties with Narain's thesis. Among these are the fact that the core of Indo-European must have arisen at least six or seven thousand years ago, whereas Tocharian can at best be pushed back to about three thousand years ago, and the fact that Tocharian is mysteriously linguistically most closely related to West European Germanic and Celtic rather than to the geographically nearer Iranian and Balto-Slavic (i.e., to *centum* rather than to *satəm* languages). Without going into excessive scholarly detail here, one thing is certain: the accurate genetic, physical, and ethnographic identification of the early inhabitants of Xinjiang, to which this project is dedicated, may well contribute not only to the solution of "the Tocharian problem" but to the question of the origins of the

Indo-European peoples as a whole, which is one of the most burning issues in both archeology and historical linguistics.

CONCLUSION

This report is intended only to serve as a preliminary account of the progress made during the first year of the existence of our project on the desiccated corpses of Xinjiang. We will have to wait approximately half a year before Dr. Francalacci and his colleagues can run through all of the time-consuming tests on the tissue samples that have already been taken. After that, comparisons will have to be made with the DNA of other known populations, both ancient and modern, before we can issue a paper on the genetic affiliations of the Xinjiang corpses. Beyond that, we will continue to take tissue samples of additional corpses as they become available and will expand our database on the genetic affiliations of the ancient inhabitants of the Tarim Basin.

The DNA research which lies at the center of the project will be supplemented by studies on the textiles and other artifacts associated with the ancient Xinjiang corpses, by examination of the teeth and other physical characteristics of the corpses, as well as by linguistic and other types of comparative research. Within three years, we expect to have published a series of articles (in Chinese and in English) on various aspects relating to the early inhabitants of Xinjiang. After five years, in conjunction with the opening of the "Mummy Museum" in Ürümqi, we will convene an international symposium dealing with broad issues in archeology, language, religion, history, and other fields affected by the revelations of this project. Distinguished authorities from around the world will be invited to attend, and a volume of papers presented at the symposium will be published. We believe that the impact of this project on cross-cultural, interdisciplinary studies at the end of this century will be enormous and will point the way toward still greater achievements in the unified history of civilizations during the next century.

BIBLIOGRAPHICAL NOTE (to supplement that of the original proposal)

For an overall picture of the distribution, excavation, and significance of the ancient desiccated corpses of Xinjiang, see WANG Binghua, "Xinjiang gu shi fajue ji chubu yanjiu (The Excavation of Mummies in Xinjiang and their Preliminary Study)," *Xinjiang wenwu (Cultural Relics of Xinjiang)*, 4 (cum. 28) (1992), 80-88, and ZHANG Yuzhong, "Xinjiang gudai ganshi de kaogu faxian he yanjiu zongshu (A Summary Account about the Finding and Studies of Xinjiang Mummies)," *Xinjiang wenwu (Cultural Relics of Xinjiang)*, 4 (cum. 28) (1992), 89-96, 88.

FUNDING

This project has been funded by the Sloan Foundation with additional support from the Office of the Vice Provost for Research and the School of Arts and Sciences Dean's Office, both at the University of Pennsylvania. We are profoundly grateful to all of them for the confidence they have shown in our efforts and the vision they have demonstrated toward the advancement of science.

A budget of all expenses incurred (\$21,345.90) has been submitted through Margaret Guinan (Business Administrator of the Department of Asian and Middle Eastern Studies) with receipts to the Office of Research of the University of Pennsylvania, which is serving as the grant administrator for this project.

Report on the

2nd Workshop on Comparative Linguistics: The Status of Nostratic: Evidence and Evaluation

Eastern Michigan University, Ypsilanti
October 21-22, 1993

IRÉN HEGEDŰS
University of Michigan

The workshop opened with welcoming remarks from Marcia Dalbey, Head of the Department of English, Eastern Michigan University.

The discussion of the Nostratic enterprise, moderated by Joe Salmons (University of Wisconsin/Purdue University), started with "The Insider's View of Nostratic" presented by Mark Kaiser, and it continued with Alexis Manaster-Ramer's presentation of "The Outsider's View of Nostratic".

Mark Kaiser (Southern Illinois University) stated that no substantive criticism of Nostratic has been put forward yet and only the principle of Nostratic has been rejected. He criticized Greenberg's mass-comparison and Bomhard's 1984 binary approach [though we know that Bomhard's Nostratic is not binary any longer]; he also mentioned examples where Bomhard truncated roots to make them match or where his phonological reconstruction is not correct. Classical Nostratic undergoes changes: data treatment is constantly refined, new (groups of) languages are included, others are discarded; for the time being the inclusion of further languages is a moot question in his opinion. Afroasiatic may indeed turn out to be a sister family (Afroasiatic entities are to be revised). Basic principles should be maintained like: working strictly in accordance with established regular sound correspondences, borrowings should be distinguished, and multilateral comparison is preferred to binary comparison in order to avoid chance correspondences.

Alexis Manaster-Ramer (Wayne State University) sounded evidently supportive of Nostratic although his was an outsider's approach to the theory. He surveyed the various attitudes toward the Nostratic hypothesis ranging from deprecative allusions (Watkins' Nostratosphere) to constructive criticism; the latter, by the way, was well demonstrated by his presentation. He cited examples where the explanatory power of the Nostratic theory becomes quite obvious (Indo-European triune velars and vocalism, Indo-European *st*, *sk* clusters in initial position only), proposed a feasible alternative for the reconstruction of Nostratic affricates and, last but not least, pointed out weaknesses like deglottalization of **q* treated as a normal process in the neighborhood of **p* in Afroasiatic, which is assumed on the basis of a handful of examples and at the same time, non-deglottalized variants are attested as well, plus there are sporadic instances of deglottalization in other environments. The case of Nostratic **sV-* "causative-desiderative" morpheme was also questioned due to the inconsistency that it is supposed to yield desiderative reflexes in Indo-European and Altaic but causative reflexes in Dravidian and Afroasiatic. Not to mention the unclear case of Dravidian voiced geminate stops that have very few cognates outside Dravidian and many examples show alternations with sonorant + stop clusters, thus the proposed Nostratic origin (stop+*H*) seems to be implausible.

The above two papers were then discussed by Brent Vine (Princeton University), who considers himself an interested onlooker. He raised questions like "who is competent to do Nostratics?" since it is such a vast field that it evidently has to be a collaborative enterprise. He thinks that long-rangers often use Pokorny's material uncritically ("fishing expeditions in Pokorny's"), although it is considered to be dated in many respects by the modern state of Indo-European studies. The reconstruction of Nostratic is lexically based, the reconstruction of morphology has its limitations since in the case of two-segment morphemes the element of chance is high. [Let me note here, that in the reconstruction of Proto-Indo-European two-segment and single-segment morphemes are also established with a high degree of certainty, furthermore in the case of Nostratic, there are several cases where a two-segment morpheme is reconstructed on the basis of evidence from 5 or 6 language families in accordance with regular sound correspondences and complete functional /semantic/ correspondence.] The Nostratic solution to the puzzle of Indo-European gutturals in Vine's opinion is considered redundant since the gutturals no longer pose a problem to Indo-Europeanists. [I wonder if there are Indo-Europeanists out there who are still bothered about the status of gutturals and would be pleased to see the Nostratic background if they knew that there is such. Yes, I am fishing for comments from experts who were not present at the workshop!]

The afternoon session, moderated by Brian Joseph (Ohio State University), was devoted to historical and methodological questions.

Vitaly Shevoroshkin (University of Michigan) gave a survey of the history and evolution of Nostratic ideas, noting that already Holger Pedersen, who coined the term and first proposed the Nostratic hypothesis, anticipated the danger of

mass-comparison. And indeed, in the adolescent period (Trombetti, Moeller, Cuny), an enormous amount of mistakes were produced. With the publications of Collinder, Menges and Poppe, however, a period of comparisons based on regular sound correspondences set in. Nostratic studies became really intensive with the research launched by Illich-Svitych, Dolgopolskij and other scholars at the Moscow Academy and it has been gaining supporters in European countries (Yugoslavia, Czech Republic, Hungary, France) and in the United States too. He called attention to the rigorous nature of the work carried out by Illich-Svitych and pointed out that actually the first two volumes of his Nostratic Dictionary should be treated separately from the first fascicle of the 3rd volume, since the latter is a collective work of the Moscow linguists who decided to publish the dictionary after the author's premature death. He also remarked that, despite several attempts, the publication of the English translation of the dictionary completed by Mark Kaiser is still in a limbo.

Joseph Greenberg's paper on "The Convergence of Nostratic and Eurasiatic" was read by Keith Denning. The title comprises the basic tenet that the significant changes in the views of Nostraticists in recent years as to what language families should be classified as Nostratic have reduced the difference between Nostratic and Greenberg's Eurasiatic. [N.B. this is only regarding the question of membership! I do not see much convergence otherwise.] There is still some incongruence even in respect of membership, because Greenberg still does not include Dravidian and Kartvelian. He considers Afroasiatic to be a sister superstock to Nostratic. At the same time, it is intriguing that Greenberg, referring to Blazhek's investigations, emphasizes that Kartvelian is connected to Afroasiatic by a significantly larger number of etymologies than to any other Nostratic branch. [Actually Blazhek mentions 65 etyma (maximum 108, including less certain comparisons) that connect Kartvelian with Afroasiatic, but this is hardly more than what he gives, e.g., for Kartvelian-Indo-European (62, maximum 100). Besides, Blazhek also called attention to the remarkable reserves of Dravidian-Afroasiatic comparisons.] In his forthcoming book, Greenberg will list 63 grammatical elements as well to support his hypothesis.

Mark Hale (Harvard University), in his discussion, outlined four criteria that govern a scholar whether to pursue a hypothesis or not:

1. Significance to others or other topics;
2. Likelihood;
3. Feasibility of yielding results;
4. Independence of scholar (running risks).

[If a hypothesis catches on (as it is seen happening to Nostratic) in any scientific field, there is more to it than just that more scholars will be positive at the end of considering the four criteria outlined above. There is a bulk of disquieting FACTS (!) that keep pushing a hypothesis toward the center of attention in professional circles.] Mark Hale's opinion supported Brent Vine's concerning the reliability of Pokorny's dictionary and also the status of the guttural problem in IE studies; his idea that it is impossible and useless to write an etymological

dictionary raised the objection of the audience.

The second day started with papers investigating the role of chance which is indeed a crucial point in establishing true correspondences, the moderator was Martha Ratliff (Wayne State University).

Robert Oswalt (California Indian Language Center) presented a talk on "The Probabilistic Evaluation of Similarities among Very Dissimilar Languages". For over 25 years, he has been developing computer aided procedures to enable a statistical determination of greater-than-chance similarity between languages of the world. He carried out intrastock and extrastock comparisons on the basis of the 100-word list. He also examined the effect of requiring 3 consonant matches and the effect of having two alternative words in each semantic slot. His calculations suggested that the branches of Altaic are distant, furthermore, the Uralic-Altaic comparison yields no significant resemblances, at least the basic vocabulary does not reveal affinity between them. It is still surprising though that a comparison between Armenian and Hungarian yielded a relatively high index in the range of 4 out of 5 or 5 out of 5 required number of matches in the manner of articulation. Such a relatively high index is easy to explain in the case of German and Finnish by language contacts. The factor of geographical position is decisive in many cases as proximity can create affinity (Indo-European neighboring branches show a higher degree of correlation).

Don Ringe (University of Pennsylvania) provoked probably the most fervent debate with his paper entitled "A Probabilistic Evaluation of Indo-Uralic". His aim was to devise a test to discard chance phenomena, to find a point beyond which it is not likely that similarities between two (groups of) languages are not random phenomena. Although he opened by commenting that Proto-Indo-European and Proto-Uralic appear to be the best candidates in the Nostratic group because the probability of their relatedness is 1:47 or 1:48, which looks better than random but is still not reliable. Actually his final conclusion was that Proto-Indo-European-Proto-Uralic is only one item above the threshold but doubted that a single item could justify their relatedness. His further assumptions were that chance similarity increases with multilateral comparison and mathematically demonstrated its disastrous failure. Most of the criticism of Ringe's investigation was directed at the linguistic data that were used as input for the calculations. This is indicative of the fact that mathematical approaches to languages are highly input sensitive, and if linguists do not agree with the selection of the input data, there is no way to convince them that the result of the investigation is correct. It was indeed strange that the calculations were based on the comparison of Proto-Indo-European and Proto-Fenno-Ugric rather than Proto-Indo-European and Proto-Uralic, and available cognates were missing from the basic list.

William Baxter, in his discussion of the session, expressed his approval of mathematical methods since they increase objectivity but he added that it is better to use them for testing rather than for solving problems. On Oswalt's paper, he commented that it presented not the traditional way a linguist would find matches, but the method is sound as far as looking

for similarities in the world's languages. Concerning Ringe's paper, he asserted that establishing a criterion derived from the distribution of consonants within the 100-word list may induce circularity of reasoning. To this Ringe added that circularity can be avoided since any size of word list can be used as input. Baxter then concluded that it is not a mathematical truth that multilateral comparison fails in all cases, especially if you find always 3 matches from 4 languages.

Mark Hale found the construction of the initial list a problem because there is no formal way of resolving the semantic issues and added that the linguistic approach has always been more productive in finding real equations.

The afternoon session concentrated on family-specific connections; Anthony Aristar (Texas A&M University) acted as moderator.

Carleton Hodge surveyed "Implications of Lislakh for Nostratic". Lislakh unites Lisramic (Afroasiatic) and Indo-European, but Hodge's comparisons seem to significantly differ from the concept of Nostratic. He analyzed non-conformist consonant sets in Semitic, then compared core vocabulary sets from the Afroasiatic branches and Indo-European to arrive at a table of sound correspondences between Afroasiatic branches and Indo-European. His resulting system of stops is strangely uniform, unlike the sophisticated set of correspondences postulated by the Nostratic hypothesis. Hodge also surveyed cases of Lislakh consonants occurring with accompanying features like aspiration and nasalization. Consonant ablaut is of special significance in Hodge's reconstructions, and he finds that prothetic *alif* can account for the presence of both *CVC(C)* and *CCV(C)* patterns of the same root in Lislakh.

Alexander Vovin (University of Michigan) represented another outsider's opinion with his paper "Nostratic and Altaic: The Level of Relationship". He discussed some of the inadequacies of Nostratic from the Altaic point of view. Altaic personal pronouns do not show regular correspondence with Nostratic. The Altaic forms with initial *m-* (1st pers. sg.) are obviously secondary, the Proto-Altaic form can be reconstructed as **bV(-n-)* 'I'. The 2nd pers. sg. with initial *t-* is attested only in Mongolian, and Proto-Mongolian **t-* does not correspond to Proto-Manchu-Tungus, Proto-Japanese, and Proto-Turkic **s-*, the initial consonant of the 2nd pers. sg. pronoun. The sound correspondence is also broken in the case of the direct object suffix, which can be reconstructed for PA as **-ba/*-bä* and thus cannot be derived from PN **-mA*. Then Vovin examined Proto-Altaic, Proto-Uralic and Proto-Indo-European lexical correspondences [Dravidian, Kartvelian, and Afroasiatic were not his concern here, though he does not discard the possibility of their affiliation with Nostratic]. He thinks that Altaic is still a stronger case for Nostratic than Dravidian or Afroasiatic. He treated the lexical correspondences with a deliberate ultraconservative approach [with an eye to achieving greater reliability of results], i.e. accepted only the comparisons that had straightforward semantics and that were supported by attested forms from more than one branch of the same family. Thus he found the following distribution of convincing correspondences: PA-PU 47, PA-PIE 44, PIE-PU 47. To account for these as sheer chance resemblances is impossible, at the same time, borrowing (from

Proto-Indo-European via Proto-Uralic to Proto-Altaic) is unlikely because then we would be faced with a phonetic development rather difficult to explain:

PIE C[+voice] > PU C[-voice] > PA C[+voice] or
PIE C[+stop] > PU C[-stop] > PA C[+stop].

His final conclusion was that Altaic is related both to Proto-Indo-European and to Proto-Uralic but for lack of the common personal pronouns, it is better not to include it in Nostratic but to consider it a separate related family.

In the discussion, William Rozycki (Indiana University) pointed out that Illich-Svitych was correct in positing Proto-Altaic initial **p*- and **k*'-, and the postulation that "r" is primary and "z" is a secondary development in Altaic can also be supported, although specialists, in the wake of Clauson, would reject these ideas. Evidently Illich-Svitych had a thorough comparativist's approach, and his Nostratic hypothesis managed to come to correct conclusions in several respects.

Numerous comments were made by the above mentioned participants, Eric Hamp and the group of graduate students present at the workshop which was concluded with a panel discussion followed by a pleasant party at Helen Arsitardry's house.

To sum up, I would say that it is very promising that the number of interested onlookers is increasing. It is even more promising that the interested onlookers take the effort and point out fallacies of Nostratic reconstructions. And it is more than promising that interested onlookers take the courage and point out the merits of the Nostratic theory.

And did we enjoy the intellectual wrestling we all were part of during those two days? For that, thanks are due to the organizing committee: Helen Arsitardry, Keith Denning, Brian Joseph, Alexis Manaster-Ramer, Martha Ratliff, Joe Salmons.

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PHYLETIC LINKS BETWEEN PROTO-INDO-EUROPEAN AND PROTO-NORTHWEST CAUCASIAN

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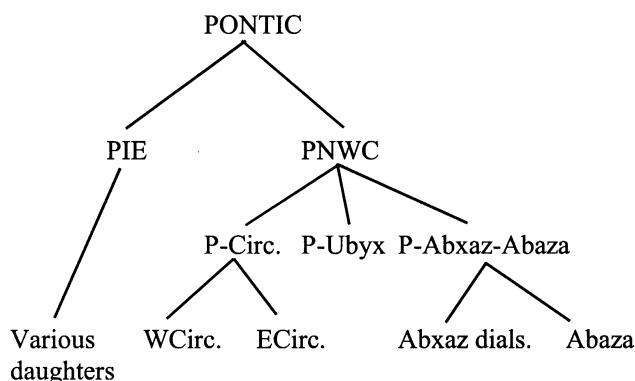
INTRODUCTION: In 1964 Paul Friedrich (1964:209), in a review of Aert Kuipers' work on Kabardian (Kuipers 1960), first made the informed suggestion that Proto-Indo-European (henceforth PIE) might be phylogenetically related to "Proto-

Caucasian."¹ Friedrich's suggestion was based on the emerging typological similarities between PIE and some of the Northwest Caucasian languages. The Northwest Caucasian look of PIE, a look which set it widely apart from any of its daughters, had first emerged under the work of internal reconstruction done by Benveniste (1935) and Lehmann (1952). The typological parallels between this early PIE and a Caucasian² language were first noticed by Aert Kuipers (1960) for Kabardian and were later taken up by W. S. Allen (1965) when he discussed Abaza vocalism. Kuipers devoted a chapter of his monograph to the parallels between PIE and Kabardian vocalism, which is very similar to the vocalism of Abaza. I myself (Colarusso 1981) have examined typological parallels involving consonantism, particularly matters regarding the so-called laryngeals of PIE and their possible typological correlates among consonants of the Northwest Caucasian languages. Typological parallels between PIE and the South Caucasian family, Proto-Kartvelian, were also put forward in the 1960s (Gamkrelidze 1967, 1966; Gamqrelize and Mač'avariani 1965; but note Kuipers 1983), suggesting that at the least PIE formed an areal grouping with the ancient Caucasian languages. In 1987, after I had presented a reconstruction of Proto-Northwest Caucasian (henceforth PNWC) (Colarusso, 1989a), Eric Hamp suggested to me (personal communication) that I endeavor to determine if PIE and PNWC might be genetically related. The following paper presents my first results suggesting that PIE and PNWC are genetically related at a phyletic level.

PROTO-PONTIC: I shall term the language from which PIE and PNWC may have descended Proto-Pontic, or simply Pontic, after the classical name for the Black Sea, *Pontus Euxinus*, which I assume was near to the homeland. In the past twenty years, the archeological work of Gimbutas (1985, 1980, 1977, 1974, 1973; see also Mallory 1989, ch. 6) has placed the most likely PIE homeland in the Northwest Caucasus. More recently, Gamkrelidze and Ivanov (1985, 1984) have argued that it lay just south of the Caucasus, in eastern Anatolia. In either case, a phyletic link with a Caucasian language is plausible. My own work in comparative mythology (Colarusso 1989b, 1984) has suggested cultural contacts with the Caucasus at a period of Indo-European unity. Whether or not Proto-Pontic is in fact Proto-Caucasian or Proto-North Caucasian, in other words, whether or not PNWC enjoys a special phyletic link with PIE not enjoyed by other Caucasian language families, rests upon further work in historical Caucasian linguistics. The genetic links between PNWC and Proto-Northeast Caucasian (PNEC) now seem quite plausible (see, for example, Čirikba 1986; Abdokov 1983). Thus, if the present study seems a worthy start, then the reader should be prepared to view PIE as one of an ancient complex of cognate languages centering about the Caucasus. In my opinion, time will show that PIE is closest to PNWC, in fact sharing certain innovations with the northern dialect area of PNWC.³

Diagram (1) gives a rough idea of the links that I shall explicitly put forward:

(1) Proto-Pontic



TIME DEPTH AND TYPES OF EVIDENCE: For such remote phyletic links as Pontic, questions of time depth and types of evidence must be addressed. While it is in principle impossible to establish exact dates based upon linguistic facts alone, I have nevertheless put forward a tentative time frame in (2) which seems to permit room enough for the type of differentiation required for both PIE and PNWC.

(2) Tentative Time Depths

1. 3,000 - 4,000 BC: Comparative reconstructed PIE
2. 5,000 - 6,000 BC: Internally reconstructed PIE
3. 2,000 - 4,000 BC: PNWC
4. 7,000 - 9,000 BC: Pontic

At such a time depth of nine to eleven thousand years, standard cognate evidence will not loom as large as in more conventional reconstructive effort. Accordingly, I shall examine three types of evidence. First, typological parallels (of phonological inventories) suggest not only an areal grouping of PIE and the Caucasus, but also show some strong defects in the PIE inventory, even as revised by Gamkrelidze, Ivanov (1973, 1972, 1967) and Hopper (1982, 1977a, 1977b, 1973). I have made modifications to the PIE inventory which make it far more plausible typologically to provide a basis for correspondence sets. Second, I examine morphological cognates, (compare Goddard 1975). Such morphotactic cognates are strong in the case of nouns, but a bit weaker in that of verbs. One of the strongest sets of data involves the homonymy of morphemes. Indeed, the ability of Pontic to explain long-standing homonyms or confusions in morphology within PIE is most striking, and it is at this stage of work that the strongest argument for the cognacy of PIE and PNWC emerges. Odd relict forms within PNWC are also explained by Pontic with much more than chance success. Many of these morphological investigations produce transparent explanations of PIE morphology at the level of Proto-Pontic. This is another very powerful argument for the cognacy of PIE and PNWC. Third, lexical cognates can be expected to be few at such a time depth. Nevertheless, a simple search found twenty items of good quality, (64)-(83). Many more await the resolution of a few details before they, too, can be established. I turn now to these various categories of evidence.

TYPOLOGICAL EVIDENCE: The phonemes of "Classical PIE" are shown in (3).

(3) Classical PIE

p	bh	(b)		m		w
t	dh	d	s	n	r	l
k	gh	g				y
k ^w	gh ^w	g ^w				

ṛ₁ (E), ṛ₂ (A), ṛ₃ (O), ṛ₄ (A, but not in Hittite in Anlaut);
vowels: e ~ o (plus tonal stress)

Typological arguments based not only upon inherent plausibility, but also upon problems in the development of the PIE system in certain of its branches (Colarusso 1981), have led to a suggested modification in (4) that makes PIE look more like a Caucasian language.

(4) New PIE (Gamkrelidze-Ivanov-Hopper), plus palatals

p ^h	(p')	b		m		w
t ^h	t'	d	s	n	r	l
k ^{hy}	k'y	g ^y				y
k ^h	k'	g				
k ^{hw}	k' ^w	g ^w				

ṛ₁ (E), ṛ₂ (A), ṛ₃ (O), ṛ₄ (A, but not in Hittite in Anlaut);
vowels: ə ~ a (plus tonal stress)

There are some unrecognized problems with (4), however, that I attempted to point out in an earlier work (1981). First, there are not enough spirants (apart from some of the laryngeals). Second, there are not enough rounded segments for a vertical-vowel system language. Such systems evolve by a rare but natural process in which the features of the syllable core are reassigned to the consonantal syllable periphery. Rounding is one of the most stable of these once so reassigned. Third, we now know enough about the effects and history of the laryngeals that any presentation of PIE needs at least feature specifications for them. Therefore, in (5) I present a typologically more accurate form of PIE, which I term "fortified PIE" and which I shall write in phoneme slashes /.../.

(5) Fortified PIE (after Colarusso 1981)

p ^h	b	-		m		w
t ^h	d	t'	s	n	r	l
k ^{hy}	g ^y	k'y				y
(k ^h)	g	k')				
k ^{hw}	g ^w	k' ^w				
q ^h	-	q'	x	ɣ		
q ^{hw}	-	q' ^w	x ^w	ɣ ^w		
			ħ	ʕ		
			ħ ^w	ʕ ^w		
		ʔ	h			
		ʔ ^w				

ə ~ a (plus tonal stress)

THE LARYNGEALS: In (5) I have given substance to the laryngeals based upon detailed considerations of PIE phonology (Colarusso 1981). I cannot repeat these here, but try to summarize my arguments by an “eightfold way.” Any phonologically realistic account of the PIE laryngeals must account for these eight facts.

First, in oldest PIE some true laryngeals produced instances of “inherently” long vowels, schematically shown in (6).

(6) Earliest Laryngeal Loss Giving “Inherently” Long Vowels:

*ē = */əɪ/ (ə₁); *ō = */əɪ^w/, */aɪ^w/ (ə₃); *ā = */əh/, */ah/ (ə₄)

perhaps also:

*ē = */ə-ə/; *ō = */a-a/ [a:] or [ɛ:] (contrast: *ā < */əh/, */ah/ = [a:])

(parallel: PNWC *h > *a)

Second, at this stage, the other segments destined to become “laryngeals” would have persisted as segments without obvious effects into the period of unity. The best candidates for such segments are in (7).

(7) Earliest Persistent “Laryngeals”

/x, ɣ, x^w, ɣ^w, h, ɦ, h^w, ɦ^w/

Third, with a shift from pharyngeal to true laryngeal in the period of early differentiation, the [+Constricted Pharynx] members of (7) would have colored vowels as in (8).

(8) Vowel-Coloring Era

/h/ > /h/, /ɣ/ > /a/ (ə₂), /h^w/ > /h^w/, /ɦ^w/ > /q/ (ə₃)

(Parallels: Abx. /h/ > /h/, /ɣ/ > /a/, /ɦ^w/ > /y^w/)

Fourth, once these segments had become true laryngeal glides, they were dropped with compensatory lengthening post-vocally, even throughout Anatolian, by means of the natural rule (9). This would have been a period of early dialect formation.

(9) Rule of Laryngeal Loss in Early Dialect Period

[-syll, +low] > [+long] / V _____. (Phonemes in (8) were lost.)
(Parallels: Circ. /ah/ > [a:], Abx. /ə/a/ > /aa/ > [a:])

Fifth, in Anatolian, some sort of segments persisted in some post-vocalic positions (Hitt. *pahhur* ‘fire,’ *mehur* ‘season,’ *sehur* ‘urine, filth’). Significantly, similar segments gave velar allophones in Italic: Lat. *senātus* ‘a council of elders,’ *senex* ‘old, aged,’ These would have been the old velar or uvular spirants, as in (10).

(10) Old Persistent “Laryngeals” of Anatolian

/x, ɣ (ə₁/ə₂), x^w, ɣ^w (ə₃)/

(Parallels: persist in Circassian and Ubykh)

Sixth, outside Anatolian, these same segments were lost with vowel-coloring and compensatory lengthening, as in (11).

(11) Loss of “Persistent Laryngeals” Elsewhere

/x/ > /h/, /ɣ/ > /a/, /x^w/ > /h^w/, /ɣ^w/ > /q/ (steps 8 and 9 again)

(Parallels: similar history in Abx.-Abz., e.g., */ɣ/ > /ɣ/ > /a/)

Seventh, there is ample evidence that all laryngeals caused source feature effects. These are of three types seen in (12). The scheme in Fortified PIE (henceforth simply PIE) neatly accounts for all of these effects in the simplest way possible.

(12) The Three Source Feature Effects

- a) Glottalization (Voicing): */-p^hɪ^w-/ > /-p^w-/ > *-bo-: Skt. *pībati* ‘he drinks,’ Ir. *ibim*, Gk. *πίνω*
- b) Voiceless Aspiration: */-t^hh^(w)-/ > Ind-Iran. *-th-
- c) Voiced Aspiration: */-t^hɣ^(w)-/ > Ind-Iran. *-dh-

Eighth, and last, laryngeals seem to have caused apparently contradictory lowering in some cases but raising in others, as in (13).

(13) Apparently Contradictory Laryngeal Effects

Gk. *θυγάτηρ*, Skt. *dúhitā*,

only [+CP] with its low, strong F can do both

(Parallels: Bzyb Abx. /ɣ/ > /a/, /ɦ^w/ > /y^w/, /y/)

This can only be understood if one realizes that this is a pharyngeal “signature” in which an acoustic assimilation produces the opposite effects of an articulatory assimilation (Colarusso 1985). Pharyngeals have a formant structure with a low and powerful first formant. This gives the impression of a high front vowel. At the same time, they are made with tongue root retraction and often with tongue root lowering, which results in approximation of the epiglottis over the adytus (opening of the larynx). Such pharyngeals or “adytals”⁴ produce low vowels by articulatory assimilation.

The phonology of the PIE “laryngeals” is complex, but can now be explained by phonological theory and must not be dispelled by elaborate arguments involving leveling and other arbitrary gestures as is now so often the case. One of the few workers who tries to utilize realistic laryngeals and follow them whither they lead is Eric P. Hamp (see, for example, Hamp 1990). This gives many of his reconstructions a distinctly Northwest Caucasian cast.

MORPHOLOGICAL COGNATES: The form and position of morphological peculiarities can be enormously useful in retrieving ancient phyletic links, so much so that this effect can

compensate for the relatively limited phonemic inventory usually associated with morphology. In the present matter, there is good case for nouns (both PIE and PNWC had N-(suffix)^m), and a less strong one for verbs. Pontic seems to have been moderately isolating, much like a NEC language. Subsequent history led the verb to be highly inflected, but in different ways in the two families: NWC: (prefix)^m-V-(suffix)ⁿ, IE: (prefix)^l-V-(suffix)ⁿ. Nevertheless, morpheme cognates are good evidence for two reasons. First, bound morphemes are unlikely to be borrowed outside the forms in which they occur. Second, PIE and PNWC morpheme cognates show a high congruence in otherwise unmotivated homonymy. In some cases, PNWC forms can explain peculiarities of PIE inflection.

SAMPLE OF NOMINAL SUFFIXES: I turn now to an actual presentation of morphological cognates, starting with the noun (and adjective) and treating primarily derivational affixes. Abbreviations in the following are: Bzh. = Bzhedukh, (W)Circ. = West Circassian, PC = Proto-Circassian, Kab. = Kabardian (East Circassian), Ub. = Ubykh, Abx. = Abkhaz, Abz. = Abaza, A-A = Abkhaz-Abaza. I have followed the usual abbreviations for the Indo-European languages. Others are: V = verb, N = noun, preV = preverbal particle. Each entry is headed by its PIE form, first in its classical representation and then, within parentheses, by its fortified one.

(14) Athematic *-Ø, (*-/Ø/): thematic *-e/o-, (*-/ə-, -a/)

- a) PIE: Gk. *ἄναξ* 'lord' < */wánakt-s/, vs. *λόγος* 'word' < */log-o-s/,
- b) PNWC: tendency of some languages to produce roots with vowelless allophones or even underlying forms: Bzh. WCirc. /s^hə/ 'brother,' /za-s^h-x^h-a-r/ all-brother-pl-abs. = 'the brothers (coll.);' /pq/ 'bone, frame' > [pqə], /w-pq-x^h-a-r/ > /p-pq-x^h-a-r/ your-bone-pl-abs. = 'your body'; Ub. /t̪x̪/ 'shoulder, back' > [t̪x̪ə], /á-t̪x̪/ the-shoulder, back, /t̪x̪ə-pqə/ back-bone, /a-t̪x̪-pqə/ the-back-bone; vs. Bzh. WCirc. /psaaχa-x^h-a-r/ word-pl-abs. = '(the) words'

(15) PIE *-(e)w- (*-/əw-/ in Adjs.

- a) PIE: Gk. *πολ-ύς* 'much,' Skt. *pur-ú-h*, Goth. *fil-u*,
- b) PNWC */-u/, */-əw/ 'predicative' and 'adverbial,' Circ. /yənəw/ big-pred., /psta-w/ all-adv., Ub. /ə-d̪ə-a-ə-by-a-w-nə/ 3-when-3-see-adv.-gerund = 'when he saw him;'
- c) Pontic */-w/.

(16) PIE *-yo- (*-/ya-/ in Abstract Adjs., *-iyo- (*-/iya-/ (see "collectives," (30))

- a) PIE: Skt. *gáv-ya-h* 'bovine,' *ásv-ya-h* 'of the horse, horse-like,' *ár-ya-h* 'Aryan,'
- b) PNWC */-ġa/ > Circ. /adəġa/ 'Circassian,' Abz. /-rġa/ 'people,' */-ya/ > WCirc. /da-a-ya/ nut-con.-one of = 'nut tree,' [-iye] vocalization of /-ya/ common in Ub. and A-A;
- c) Pontic */(rə)ġa/ 'people' (see (30)), */-ya/ 'the one of,' adjectival suffix.

(17) PIE *-yo- (*-/ya-/ opposition in other terms

- a) PIE: Lat. *alius* 'the other,' Gk. *δεξι-ός* 'the right one,' Goth. *niu-ji-s* 'the new one,'
- b) PNWC */-g^ya-/ 'and': Ub. /-g^ya/ 'and,' Circ. /-əy/ 'and' (of clauses), Abz. /-g^y-/ 'and' (preV.);
- c) Pontic */-ge/ 'and' (of pairs).

(18) PIE *-en- (*-/ən-/ used in oblique cases

- a) PIE: Goth. *guma* 'man,' *gumin-s* 'gen.,' Lat. *homō*, *homin-is* id.,
- b) PNWC */-n/ or */-m/ oblique case, genitive formation: Circ. /χ'ə-m ə-q^w/ man-obl. his-son,
- c) Pontic */-m/, (rather than */-n/, because the former is typologically more marked, so the shift */m/ > */n/ may be explained as a typological simplification).

(19) PIE *-no- (*-/na-/ secondary NPs

- a) PIE: Lat. *lūna*, Praenestian *losna* < */lowks-no-, Av. *roučah* 'light, lamp'; Skt. *pūr-ṇá-h* 'something full,'
- b) PNWC */-nə-/ frozen derivational suffix in Circ.: Bzh. /č'aš'-nə-nəq^wa/ night-/nə/-half = 'midnight,' so-called "syllabified connective" in /λa-nə-s^ht^ha/ 'scissors,' /s'a-nə-ya/ know-/nə/-ness = 'knowledge' (so, by this last form /-nə/- cannot be an old genitive),
- c) Pontic */-na-, */-nə-/.

(20) PIE *-eno- (*-/əna-/), *-ono- (*-/ana-/ participle in Germanic

- a) PIE: Gmc.: Goth. *itan* 'eaten,' *bit-an-s* 'killed,'
- b) PNWC: Abz. /-ən/ "pro-tense," replaces tense in concatenated or subordinated ("dependent") forms: /s-č'a-n/ I-eat-dep.; Ub. /-nə/, /-na/ old gerund, /a-la-sə-nə ... ə-d̪ə-a-ə-by-a-w-na ... ə-yə-q'a-q'a she-there-sit-ger. ... him-when-she-see-adv.-ger. ... it-she-say-past = 'she was sitting there ... when she saw him ... [and] she said,'
- c) Pontic */-əna/ old participle ending.

(21) PIE *-(t)er (*-/t^h)-ər/ old kinship suffix

- a) PIE **swesor* > Lat. *soror* 'sister,' E. *sister*, Arm. *k^hoyr*, Pers. *x^wāhar*, **p(ə)tér(s)* > Gk. *πατήρ*, Skt. *pitā*, Lat. *pater*, Arm. *hayr*, Ir. *athir*, Goth. *fadar* 'father,'
- b) PNWC */X-t^h-ər/ X-be-part(icipie) = 'the one who is X,' **X-ər X-pt.* = 'the one who is X,' 'the X,'
- c) Pontic */-t^h-ər/ -be-part., */-ər/ -part.

(22) PIE *-er (*-/ər/) in nom.-acc., sg., neut., *-en (*-/ən/) in obliques

- a) PIE: Skt. *údhār* 'breast,' *údh-na-h* gen.,
- b) PNWC */-ə/r in abs(olutive) (if neuter [-agentive], one will not have an ergative role), */-əm/ or */-ən/ in obl(ique) cases: Circ. /χ'ə-r/ man-abs., /χ'ə-m/ man-obl.; Ub. /tát/ 'man(abs.),' /tát-ən/ man-obl.,
- c) Pontic */-ər/ abs., */-əm/ obl., (note (17, c)).

(23) PIE **-yes-/*-yos-* (**-/yəs-/* or **-/yas-/*) Comparative; **i-s-t(h)o-* (**-/y-s-t^h-a-/* or **-/y-s-da-/*) Superlative

1. Comparative:

- a) PIE: Skt. *svá-d-īyas-* 'sweeter,' Gk. ἡδιώ id.,
- b) PNWC **-/y-č^h-* -dir(ecti)on-be-excessive > Bzh., WCirc. */-š^{hy}-* 'excess,' Ub. */ča-/* comp.;

2. Superlative:

- a) PIE: Skt. *svá-d-iṣṭha-h-*, but Gk. ἡδ-ιστο-ς (**ἡδ-ισθο-ς* odd!),
- b) PNWC superl. = comp. + 'exactly' **-/y-č^h-(də)da/*, whence > **-/y-č^h-t^ha/* > PIE **-y-s-to-* or **-/y-ž^h-da-/* > PIE **[-i-z-dho-]* (**-/i-s-dho-/*),
- c) Pontic **-/(-y-a)-č^h/* (-dir.-dat(ative)-) be excessive (PIE **-yas-*), **-/(-y)-č^h/* (-dir.-) be excessive (whence the Circassian form), **/č^h-a/* be excessive-dat. (whence the Ubykh form).

(24) PIE **-ter-* (**-/t^hər-/*), **-tel-* (**-/t^həl-/*) Agents

- a) PIE: Gk. γενε-τήρ, γενε-τώρ, OCS. *bljustelji* 'observer'; NB: Hitt. has only **-l-*,
- b) PNWC: Abz. */-la-/* instrumental, */qāc'a-la/* man-instr. = 'by means of the man,' in the north, this is *-(r)a-/*: Kab. */wa-r-k^ya/* you-instr.-instr. = 'with you(r help),'
- c) Pontic **-/(-t^hə)-l-/* instrumental: PIE **-/t^hə-/* is probably an innovation based upon the extension of the genitive as an oblique case (cf. Abz. */qāc'a-ta/* man-gen. = 'of, from the man'); note, part of PIE also shares an isogloss (**/l/* > */r/*) with northern PNWC.

(25) PIE **-tro-* (**-/t^hra-/*), **-tlo-* (**-/t^hla-/*), **-dhro-* (**-/dra-/*), **-dhlo-* (**-/dla-/*) Instrumentals

- a) PIE: Skt. *mán-tra-h-* 'prayer,' Lith. *(pa-)men-klas* < **men-tila-* 'monument,' Lat. *pō-culum* < **pō-tlo-m* 'drinking cup,' OIr. *cé-tal* < **kan-tlo-* 'song,' OHG *sta-dal* < **sta-pla-* 'barn,' Gk. γένε-θλο-ν 'race, descent,' ἄρο-τρο-ν 'plough,' Lith. *ár-kla-s* 'plough,' Czech *rá-dlo* id.,
- b) PNWC **-/la-/* (same as (24)), Abz. */haq^w-la/* rock-instr. = 'with the rock'; Circ. */zə-rə-z/* one-by-one, Bzh. WCirc. */ə-z-a-r-a-ḷaḡ^wə-ḡa-x^h/* 3-reciprocal-dat.-instr(umental)-dat.-see-past-pl. = 'they saw one another,'
- c) Pontic **-/t^ha-la-a/* -gen.-instr.-dat. (like Circ. reciprocal) > PIE **-/t^hla-/*, **-/dla-/* (with assimilation), or **-/t^hra-/*, **-/dra-/* in more northerly form.

(26) PIE **-men-* (**-/mən-/*) nominal action affix

- a) PIE: Skt. *bhár-ma*, *bhár-ī-man-* 'action of carrying,' Gk. φέρ-μα,
- b) PNWC: Kab. */wə-ma/* strike-/ma/ (old affix) = 'wooden club for hammering,'
- c) Pontic **-/m(a)n-/*.

OTHER ENDINGS: I turn now to some other endings, such as participles, abstracts, cases, and such.

(27) PIE **-ent-*, **-ont-*, **-nt-* (**-/ənt^h-/*, **-/ant^h-/*, **-/nt^h-/*) Active Participle

- a) PIE: Lat. *dēns*, *dentis* (gen.) 'tooth' (lit. 'the eater'); Gk. ὀδούς, ὀδόντος (gen.); Lith. *dantis*; Goth. *tunþus*,
- b) PNWC: Abz. */-n/*, Ub. */-nə/*, */-na/* old participles, plus Circ. */-t^h/* durative (distributed) tense,
- c) Pontic **-/(-a)n-t^h-/* 'participle-durative.'

(28) PIE **-we/os* (**-/wə/as/*), **-we/ot* (**-/wə/at^h/*), Perfect Active Participle

- a) PIE: Gk. *-(F)ός* neut. nom.-acc., *-(F)οτ-ος* gen.,
- b) PNWC **-/w(a)-/* aspect sfx., Kab. */-w-/* progressive asp., */s-a-w-šš/* I-pres.-prog.-eat = 'I am eating,' Abz. */-w(a)-/* id., */s-č'(a)-w(a)-n/* I-eat-prog.-past = 'I was eating,' Abz. */-w(a)-z-t-ən/* of dependent past durative, */s-č'(a)-w(a)-z-t-ən/* I-eat-prog.-past-dur.-dep. = 'that I was eating (for a period of time),'
- c) Pontic **-/wa-z-t^h-/* > PIE **-/wast^h-/* > **-/wos-/*, **-/wot-/*, by dialect splitting.

(29) PIE **-ā*, **-y-ā* (**-/əā₂/*, **-/y-əā₂/*) Feminines and Abstracts

- a) PIE: a long scholarly history examining the homonymy of feminines and abstracts,
- b) PNWC **-/xa/* 'woman' > Ub. */xa-y^wa/* you-sfx. = 'you (free woman)'; **/w-xa-šəmc'á/* > Bzyb Abx. */(-á)ḥ^wšša/*, Ashxarwa Abx. */ḥ^wsəsa/*; PNWC **/ʔa/* 'hand' > PC */q'a/* (N, V) > **-/qa-/* (preV), **-/ya/* (N-sfx.) 'hand' or 'belonging to,' 'being in hand,' or '-ness' (=abstract suffix),
- c) Pontic **-/xa/* 'feminine' and **-/q'a/* abstract suffix have coincided in PIE.

(30) PIE **-yā* (**-/yā₂/*) Collectives

- a) PIE: Gk. φρατρία, OCS. *bratřja* 'fraternal groups,'
- b) PNWC: old collective in Abz. */wa-ʔa/* man-coll. < **/ʔ^wa-ʔa/* (cf. Abz. */(-a)ʔ^wə/* 'man,' Bzh. WCirc. */(-š^hə)wə/* '(horse-)man,' Ub. */wəwə/* 'devil' (< **/wə-də/* man-derivational-sfx.) < PNWC **ḡu-*, **w-ḡə-* 'man.' Whence also Abz. */-ʔa/* 'coll.' (< **-/ḡa/*) and the */ḡa/* in PNWC **/rəḡa/* 'people,' Circ. */adəḡa/*, Ub. */a-dəḡá/* 'Circassian,' Abz. */-rʔa/* 'people,'
- c) Pontic **-/ḡa/* > PNWC **-/ḡa/* 'man(kind),' 'collective,' Pontic **-/ḡa/*, **-/aḡ/* > PIE **-/yaḡ/* (**-yā₂*) > **-/yā/*, by leveling.

(31) PIE Cases

	PIE	NWC
acc	<i>*-m/-n</i>	<i>/-m/</i> (obl. in Circ.) <i>/-n/</i> (obl. in Ub.)
gen/abl	<i>*-(ə/a)s</i> (athem)	<i>*-/š/</i> (old genitive)
gen	<i>*-o-s(y)o-</i> (them)	<i>*-/š-y-a/</i> > <i>/-š^y/</i> obl. of pronouns in WCirc.
abl	<i>*-ō</i> (them)	Ub. <i>/-x^ya/</i> , A-A <i>/-x^ya/</i> 'place' or

		<i>*/a-a/</i> vowel-in, as with final <i>/a/</i> in Circ. <i>/-y-a-pā-a/</i> -3-dat.-look-in
dat	<i>*-əy-</i>	<i>*/-y-(a-)/</i> dir.-(dat.-), Circ. preV
loc	<i>*-i</i>	Circ. preV <i>/-y-/</i> direction, old Bzh. WCirc. dat. of pronouns <i>/-y/</i>
instr	<i>*-ē, *-ō</i>	<i>*/-ə-a/</i> > <i>*/-ā/</i> (?), <i>*/-a-a/</i> > <i>*/-ā/</i> , with <i>*-a</i> the same as in the thematic ablative

Pontic

acc	<i>*/-m/</i>	'oblique case'
gen	<i>*/-š(-y-a)/</i> or <i>*/-y-š-a/</i>	'old oblique of pronouns or old genitive'
abl	<i>*/-y-(a)-x/</i>	-dir.(dat.-)place
dat	<i>*/-y-a/</i>	-dir.-dat.
loc	<i>*/-y/</i>	-dir.
instr	<i>*/-a/</i>	-dat.

(32) Demonstratives

(1) *anaphora*

- PIE **/s-a/* nom., sg., **/t^h-a/* oblique,
- PNWC **/śa/* 'what,' **/t^hə/* 'where,' Bzh. WCirc. */śa/*, */śəd/* 'what,' */t^həda/* 'where,'
- Pontic **/ś-a/* what-dat., **/t^h-a/* where-dat.

(2) *deixis*

- PIE **/-w-/* > Skt. *asau*,
- PNWC **/wə-/* 'that (near hearer),'
- Pontic **/wə-/* deixis (near hearer).

(3) *relative*

- PIE **/ya-/*,
- PNWC **/yə-/*, Bzh. WCirc. */yə-/* optional absolutive verbal index, Abx.-Abz. */y-/* relative initial verbal index,
- Pontic **/y-a-/* old relative particle-dat.

(33) Personal Pronouns

	PIE		PNWC
	<i>nom.</i>	<i>obl.</i>	
sg. 1	<i>*egō</i> (<i>*/ʔək'-ŋ^w-/</i>)	<i>*(e)m</i>	<i>*/m-/</i> 'that near me' < Pontic <i>*/ʔə-k'-/</i> , <i>*/ʔə-m-/</i> ,
sg. 2	<i>*tu</i> (<i>*/t^hw-/</i>)	<i>*tew-/</i> <i>*/tw-/</i> <i>*/t-</i>	<i>*/w-/</i> < Pontic <i>*/t^hw-/</i> , cf. A-A <i>*/b-/</i> 'you (fem.)' < <i>*/tb-/</i> < Pontic <i>*/t^hw-/</i> by regular A-A sound developments

	<i>nom.</i>	<i>obl.</i>	
p. 1	<i>*ways</i>	<i>*nās/*nas</i>	(recent innovations in NWC: Bzh. WCirc. <i>/t-/</i> , Ub. <i>/š^y-/</i> , A-A <i>/ḥ-/</i>),
pl. 2	<i>*yus</i>	<i>*wās/*was</i>	PNWC <i>*su-</i> , <i>*w-sə</i> , WCirc. <i>/ś^w-/</i> ; Ub. <i>/s^w-/</i> , Bzyb Abx. <i>/ś^w-/</i>
	Hitt. <i>šumeš</i> , OIr. <i>swés</i>		

Pontic **/swə-/* > PIE **swə-*, is shaped by 2nd sg., but **swə-* > late PIE **wōs/*wos* is shaped by 1st pl.

PREVERBS (OLD NOUNS): Remarkably, the preverbs show some strong parallels between PIE and PNWC.

(34) PIE **perǵ-* (**/p^hər-ʔ-/*) 'before'

- PIE: loc. **per-ǵ-i* > Gk. *πέρη*; gen.-abl. **pr-ǵ-o-* > Gk. *πάρος*; instr. **prō-*, **pro-* > Lat. *prō-*, *pro-*,
- PNWC **/p^ha-r-(a-y-)/* front-along-(dat.-dir.-); E. *ford* is usually grouped here as a verbal form, but cf. Bzh. WCirc. */-pḫə-rə-pšə-/* -through-along-crawl = 'to crawl through something (such as underbrush),'
- Pontic **/p^həxə-rə-/* through-distributed > PIE **perǵ-* (with metathesis of **-x-* and **-r-*); Pontic **/p^hə-rə-/* front-distr. > PIE **per(ǵ)-*.

(35) PIE **en-* (**/ʔən-/*) 'interior'

- PIE loc. **en-i* > Gk. *ἐν*, *ἐν*, Goth. *in-*,
- PNWC: Abz. */-n-/* in */n-c'a-ra/* in-place-inf. = 'to place inside'; PNWC **/ʔ(a)-/* > Ub. */q'á/* 'hand'; WCirc. */-q(a)-/* preV denoting 'action in hand'; A-A */-q'a-c'a-/* -hand-set = 'to do,'
- Pontic **/(ʔə)n-/* (hand)-in-.

(36) PIE **et-* (**/ʔət^h-/*) 'without, outside'

- PIE: loc.: Gk. *ἐξ*; with deictic **/w-/*, Goth. *ut-*, Skt. *ut-*,
- PNWC: Abz. */-t-/* 'from inside out,' 'from below upwards,' */t-ga-ra/* out-drag-inf. = 'to drag something out,'
- Pontic **/(ʔə)t^h-/* (hand)-out-.

(37) PIE "final **s*"

- PIE: Gk. (Dor.) *ἐνς*, (Att.) *εις*, Goth. *ut-*, *us-*,
- PNWC old oblique in **/-š/*,
- Pontic **/-š/* old oblique on nominal ancestors of preverbs.

PARTICLES: Particles are so short as to make comparative study extremely difficult, but even here, two forms show such close parallels between PIE and PNWC that they can be taken back to Proto-Pontic.

(38) PIE **r* 'and'

- a) PIE: Gk. ἄρ, ῥά, ἄρα, Lith. *ir*,
- b) PNWC */-*ra*/: Circ. /-*ra*/ 'and,'
- c) Pontic */-*ra*/.

(39) PIE *-*ge* (*-/*k'*ə/) 'because,' 'terminus'

- a) PIE: Gk. -γε, Hitt. -*k*, Goth. *mi-k* 'to me,' *au-k* 'because' ('from that'),
- b) PNWC */-*y-k'*/ -dir.-instr., PC */-*k'*^y*a*/ > WC /-*k'*^y*a*/, /-*g'*^y*a*/, /-*č'*^y*a*/,
- c) Pontic */*k'*ə/ 'because, arising from, issuing from.'

VERBAL DESINENCES (CHANGE VOWEL GRADE OF STEM) AND SUFFIXES:

Even though the subsequent history of the verb in PNWC tended toward massive prefixation and that of PIE tended toward suffixation, there are numerous parallels between the two families so that a strong case for a Pontic verb can be made.

(40) Athematic : Thematic

- a) PIE: (athem.) Skt. *ád-mi* 'I am eating,' : (them.) *rod-ā-mi* 'I am crying,'
- b) PNWC:
 - i) basic verb athem. (?) */-*t^h*-/ 'to be,' */-*w-k'*-/ -valence-kill-, Ub. /ø-s-k^w-q'á/ it-I-kill-past = 'I killed it,'
 - ii) verbs with stem-final /a-/ showing thematic conjugation: WCirc. /psaa^há/ 'word,' /t-zara-psa^há-a-ya/ we-reciprocal-converse-th. v-past = 'we talked,'
- c) Pontic *CVC-afx.* forms vs. *CVCa-a-afx.* forms with thematic vowel.

(41) Intensive Reduplication

- a) PIE: Skt. *dediṣ-te* 'he teaches and teaches,' OCS. *gla-gol-jq* 'I speak,'
- b) PNWC: WCirc. /-śa-śa-/ -fall-fall- = 'to fall (as of leaves)' (old "athematic"), /-la-la-/ -hang-hang- = 'to dangle,'
- c) Pontic *CVC-* > *CV-CVC-*.

(42) PIE themes with *-ē-, *-ō-, *-ā-

- i):
 - a) PIE **men-* (*-/mən-/) 'to have in spirit,'
 - b) P-A-A */-ma-/ 'to have, to do' (now only in prohibitive form),
 - c) Pontic */-mən-/ , */-man-/;
- ii):
 - a) PIE *-*mn-ē-* (*-/mn-əʔ-/) stative sense: OCS. *mīnēti* 'he thinks,' Gk. μανῆ-ναι 'to be maddened,'
 - b) PNWC */-q'a-V-/ -horizon-V- = 'V that is of interest to the speaker,'
 - c) Pontic */-ʔa-V-/ , */-V-əʔa-/ 'in hand,' affix for action of intimate concern to the speaker;

iii):

- a) PIE *-*mn-ā-* (*-/mn-əʔ₂-/ iterative = 'to recall,'

- b) PNWC */-x-/ iterative, Abz. /n-c'a-x-ra/ in-place-again-inf.,

- c) Pontic */-mn-əx-/;

iv):

- a) PIE *-*mn-ō-* (*-/mn-əʔ₃-/): Gk. *ἑαλω-ναι* 'to be taken,'
- b) PNWC (?) */-q^wa-/ 'excess,' WCirc. /-šxə-ʔ^wa-/ eat-too much.

(43) PIE *-*eyo-* (*-/əya-/), *-*ī-* (*-/yʔ-/), *-*y-* (*-/y-/) Causative/Iterative

- a) PIE: Ved. *sād-āya-ti* 'he made him sit, he sat him down' ("inherently" long vowel pattern),
- b) PNWC: Ub. /-aay-/ 'again, finally' (NB: /aa/ [a:] perhaps involved with root lengthening in PIE),
- c) Pontic */-aya-/ , */-əya-/ iterative, resultative.

(44) PIE Sigmatic Aorist, *-s-

- a) PIE: Ved. *véṃ-s-i* 'I have won,' Gk. *ἔπαι-σ-α* 'he has stopped,'
- b) PNWC */-z-/ : Circ. /-z-/ stative or accomplished past particle with past pt., Bzh. Circ. /fa-d-əy-z/ for-be like-past, pt.-completely = 'he was completely like him'; Abz. /s-č'(a)-w(a)-z-t-ən/ I-eat-prog.-past-distr.-dep. = 'that I was eating (for an interval),' and other forms,
- c) Pontic */-z-/ past ending of full effect.

(45) PIE *-Infix Presents (*CVC-C-* > *CC-nə-C-*)

- a) PIE: Hitt. *ḫar-k-* 'perish, be destroyed,' *ḫar-ni-ik-zi* 'he destroys,' *ḫar-ni-in-kán-zi* 'they destroy,'
- b) PNWC: Ub. /-n/ dynamic present /ø-fa-ø-bzat^w-ə-n/ it-down-he-hang-pres. = 'he is hanging it,'
- c) Pontic */-n/ *n*-infix dynamic present.

(46) PIE Primary Active 3rd Plurals in *-n-

- a) PIE 3rd sg. *-*ti* (*-/t^hi/), 3rd pl. *-*(e/o)-n-ti* (*-/ə/a)-n-t^hi/),
- b) PNWC: Ub. 3rd pl. /-na-/ , /ø-fa-ø-bzat^w-ə-na-n/ them-down-he-hang-pl.-pres. = 'he is hanging them,'
- c) South Caucasian: Old Georgian /km-n-na/ make-pl.-3rd, past = 'he made them,'
- d) Pontic */-na-/ third person plural infix of actives.

(47) PIE Middle Voice in *-dh- (*-/d-/)

- a) PIE: Gk. (Dor. and Hom.) *ἔσθ-θ-ω* < *ἔδθ-θ-ω 'I am eating,' (Skt. *ád-mi*), Goth. *wal-d-a* 'I dominate,' OCS. *vla-dq*,
- b) PNWC: Abz. optative of self-interest /s-č'a-n-da/ I-eat-dep.-middle = 'O, if I could eat!,'
- c) Pontic */-da-/ self-interest forms.

(48) PIE Perfects in *-k- (*-/k^h-/), *-g- (*-/k'-/), *-gh- (*-/g-/)

- a) PIE: Gk. *ἔθη-κ-α* 'he placed it,' Phrygian *αδ-δα-κ-ετ* 'he has made it,'
- b) PNWC */-q'a/ past: Ub. /-q'a/, WCirc. /-ya/, ECirc. (Kab.) /-aɣ/ > [a:],

- c) Pontic */-q'a/, */-ya-/ with dialect variation just as in NWC today.

(49) PIE Optative in *-yē- (*/-yā-/-, *-yā- (*/-yā-/-)

- a) PIE *es- (*/-əs-/-) 'to be': Skt. *ás-ti* 'he is,' (*/-s-yā-/-) (>) Skt. *s-yā-t* 'may he be,'
 b) PNWC */-əy/ optative, concessive: Kab. /ə-ḡ^waana-ma-əy/ 3-warm-if-even = 'even if it be warm,'
 c) Pontic */-yā-/- optative, 'even.'

(50) Primary, Active, Present, Athematic *-i (*/-y/-)

- a) PIE 1st sg. */-m-i/ 1st pl. */-məs-i/
 2nd sg. */-s-i/ —
 3rd sg. */-t^h-i/ 3rd pl. */-(ə/a)nt^h-i/
 b) PNWC */-y-/- present: Abz. dynamic /s-ḡ^wə-y-t'/ I-write-pres.-def. = 'I am writing,' /s-ḡ^wə-t'/ I-write-def. = 'I wrote,'
 c) Pontic */-y-/- active present affix.

(51) PIE Relic Impersonals in *r

- a) PIE 3rd pl.: Skt. *śe-re*, Av. *sōi-re* 'they are lying down,' Brythonic impersonal: Armorican Breton *new gueler* 'one does not see me,' Passive: OIr. *berir* 'he is carried,' Umb. *ier* 'one goes,' Lat. *ī-t-ur* 'one goes,' Middle: Toch. A *kälytär* 'he stands,'
 b) PNWC */-ra/ optional present: Kab. 3rd pl. (occasional impersonal nuance), /ma-a-k^wə+a(r)/ 3-pres.-go+intrans.(-pres.) = 'they are going'; interrogative force in non-affirmatives /ə-y-a-g^ya-ra/ he-it-dat.-read-pres. = 'is he reading it?,' /ə-y-a-g^ya-r-q^wə/ he-it-dat.-read-pres.-not = 'he is not reading it' (cf. /ə-y-a-g^ya-ś/ he-it-dat.-read-affirmative = 'he is reading it'), Shapsegh WCirc. 3rd past intrans. /rə-k^wə+a-ay/ 3-go+intrans.-past = 'he went,' A-A 3rd pl. non-initial verbal index /-r-/, /yā-q'a-r-c'a-t'/ it-hand-they-set-def. = 'they did it,'
 c) Pontic */rə-/ third, plural, indefinite person, */-ra-/ non-assertive-present.

(52) PIE "s-Movable"

- a) PIE */sp^h-/~*/p^h-/: Skt. *spās-* 'to spy' ~ *pásyati* 'he sees'; */st^h-/~*/t^h-/: Goth. *stauta* 'I strike' ~ Skt. *tudāti* 'he strikes'; */sk^h-/~*/k^h-/: OHG. *skeran* 'to shear, clip' ~ Gk. *κείρω* 'I shear'; OHG. *smeltzan* 'to melt' ~ Gk. *μέλδομαι* 'I melt,' OHG. *malz* 'malt'; */s-/~*/w-/: Gk. *ἔλκω* (< *ἑλκω) 'I drag, pull,' Lat. *sulcus* (< **solkos*) 'furrow' ~ Lith. *velkù*, OCS. *vlěkq* 'I pull' (< PIE */swəl-k^h-/),
 b) PNWC */(-y)-ḡ^h-/ > PC */-y-ḡ^h-/ -dir./3-deixis- > PC */-ḡ^{hy}-/ 'there,' entirely optional on verbs, Ub. /-la-t^w-/-deixis-be- = 'to be there, exist,'
 c) Pontic */-ḡ^h-/ 'there' (deixis on verbs).

(53) Personal Endings — not much, but note:

- a) PIE *s-loss: Gk. ἡδ-ῖ-ω 'sweeter' (< **swed-íyo-s*), Av. *mā-prō* 'prayer' (< **man-tra-s*), Gk. *πατήρ* (**pāt-ér-s*);

PIE thematic 1st sg. primary active present *-ō (*/-ā/) < *-o-s (*/-a-s/ ?),

- b) PNWC */-s-a-/ -I-pres. (active)-: Bzh. WCirc. /s-a-tḡā+a/ I-pres.-write+intrans. = 'I am writing,'
 c) Pontic */-a-s/ thematic vowel-first person.

(54) PIE Futures in *-(ə)s(y)e-/*-(ə)s(y)o- (*/-(ə)s(y)ə-/- or */-(ə)s(y)a-/-)

- a) PIE: Skt. *vak-ṣ-yā-mi* 'I will speak,' Gk. *λείπω* 'I will leave,'
 b) PNWC */-š-/- > Abz. /-š-/- fut., /-č'(a)-w(a)-š-t'/ I-eat-fut.-def. = 'I will eat'; */-x-š-/- > Abz. stative futures, /s-bzəy-x-w-š-t'/ I-good-afx.-prog.-fut.-def. = 'I shall be good,'
 c) Pontic */-š-/- future-, */-x-š-/- stative-fut.-.

(55) PIE Intensives in *-sk(e/o)- (*/-sk^h(ə/a)-/)

- a) PIE: Hitt. endings -ški-z-i -intensive-3 sg.-present, -škan-z-i -intensive-3 pl.-pres.
 b) PNWC */-šxə/ > PC */-šx^wə/ > Shapsegh WCirc. /-f^wə/, Natukhay Circ. /-š^wx^wə/, Bzh. WCirc. /-šk^wə/, Kab. /-šx^wə/, confined to nouns, but note other adjectives, such as /ba/ 'much,' that can play adverbial roles: Kab. /sə-q'a-mə-k^w+a-žə-fə-nə-w-ta-ba/ I-hor.-not-go+intransitive-back-able-fut.-def.-irrealis-much = 'I shall not be able to go back again then, even so!,'
 c) Pontic */-šxə/ > PIE */-sk^hə-/ (with special cluster development, as seen also in Circassian).

(56) The Augment *e- (*/-ə-/-)

- a) PIE */-ə-/- marks the past, as in Ved. Skt. *á-bharat* 'he carried,' Gk. (Hom.) ἔ-φερε, but it attracts stress as though it were originally a word, as in Gk. *παρ-έ-σχον* (**πάρ-ε-σχον*),
 b) PNWC */-(a)/ > PC */q'(a)/ > Bzh. WCirc. with preV loss of ejective feature /ə-qə-w-a-s-t^hə-y/ it-hor(izon of interest)-you-dat.-I-give-past = 'I gave it to you' (accomplished transfer of ownership expressed through /-qə-/-), Abx. /-q'a-c'a/-hand-set- = 'to do,'
 c) Pontic */-(a)/ '(in) hand,' originally an independent adverb before the verb denoting accomplishment of action. The development in PIE suggest links between it and northern (Proto-Circassian) PNWC.

STEM FORMATION (à la Benveniste): One of the oldest patterns in PIE is that of vowel-loss in roots or stems as suffixation proceeded: C₁VC₂-C₃; C₁C₂-VC₃; C₁C₂-C₃-VC₄ (Benveniste 1935). Parallel to this is the vowel reduction pattern of Circassian morphemes in pre-root positions in verbs, as in (57).

(57) Pre-Root Vowel-Reduction in Bzhedukh West Circassian

- a) /wə-qa-s-ḡay^wə-y/ you-hor.-I-see-past = 'I saw you,'
 b) /wə-qə-əh-də-s-ḡay^wə-y/ you-hor.-3-pl.-with-I-see-past = 'I saw you with them,'

- c) /wə-qə-əh-də-s-əy-γə-λay^wə-γ/ you-hor.-3-pl.-with-I-he-cause-see-past = 'he showed me you together with them.'

If the pattern in (57) is old and is in any way related to the PIE patterns, then in some verbs, one might expect C₁VC₂-to be preverbal components, while C₃ proved to be a root. In the conventional view, one should expect etymologies for C₃ as suffixes to a root. Etymologies for C₃ have proven to be hard to find (though not for C₄). Taking the PIE and Circassian patterns to be related, one might look for cases, therefore, in which C₃ was the root. In (58) and (59), there may be just such a pair (Benveniste 1935:151).

- (58) PIE **tér-ə₁*- (**t^hér-ə₁*-): Gk. τέρ-ε-τρον 'borer' vs. **tr-éə₁*- (**t^hr-ə₁*-): Gk. τρή-σω 'I bore.'

- (59) PIE **tér-ə₂*- (**t^hér-ə₂*-): Hitt. *tarh-* 'to conquer' vs. **tr-éə₂*- (**t^hr-ə₂*-): Lat. *trāre* 'to cross upon,' *trāns* 'across.'

It is hard to imagine what root **t^hər-* in conjunction with what enlargements would produce the resulting meanings in (58) and (59). If the first morpheme is not a root but rather a preverb, however, while the enlargements are in fact distinct roots, then (58) and (59) would not only present a plausible situation, but would find straightforward cognates in PNWC, (60)-(63).

- (60) PNWC **/-t^hə-rə-w-?ə-* -surface-distr.-valence-stick = 'to stick into a surface,' WCirc. */-t^h(-y-a)-?^wə-* -surface(-dir.-dat.)-stick = id.

- (61) Pontic **/-t^hə-rə-?ə-* -surface-distr.-stick- > PIE **t^hér-?-*, **t^hr-ə₁?*-.

- (62) PNWC **/-t^hə-rə-ħa-* -surface-distr.-enter- = 'to enter on something or someone,' 'to conquer' (NB: PNWC has the same range of senses for this form as PIE), WCirc. */-t^h(-y-a)-ħa-* -surface(-dir.-dat.)-enter- = id.

- (63) Pontic **/-t^hə-rə-ħa-* -surface-distr.-enter- > PIE **t^hér-ħ-*, **t^hr-ə₁ħ-*.

Many of the odd "homophonous" roots or semantically skewed derivations of the sort of (58) and (59) may be amenable to a solution of this type. Further work in this area promises to reveal some of the more obscure cognates between these two families as well as to throw light upon some of the more difficult laryngeal developments within Indo-European history.

CONVENTIONAL COGNATES: In the following, I conclude this study with a list of some of the best and simplest cognates of a conventional sort. While they do not bulk large in this study because of the time depth for Proto-Pontic, they

nevertheless can be found. Many are of a very striking and forceful character, both phonologically and semantically. In these, I give first the Pontic reconstruction, followed by the PIE and then the PNWC histories.

- (64) 'fire' ('that which descends [from heaven], i.e., 'lightning')

- a) Pontic **/p^ha-χ^wə-r/* down-fall-abs./ger. = 'that which falls,' **/p^ha-χ^wə-n-i/* down-fall-obl.-dat. = 'in the fire,'
b) PIE **/p^hax^wə-r/* > Hitt. (nom.-acc.) *pahhur* 'fire,' (dat.) *pahhweni* 'in the fire,'
c) PNWC **/-p^ha-/* 'down,' 'to descend' > WCirc. */-p^ha-λaλa-/* -down-dangle-, Ub. */-fa-/* 'to ignite,' **/-χ^wə-/* 'to fall' > WCirc. */-fə-/*, ECirc. */-χ^wə-/*.

- (65) 'period of time,' 'season,' 'day'

- a) Pontic **/məšə-(w)/* interval-predicative,
b) PIE **/māx^wə-r/* season-abs. > Hitt. *mehur* 'day, season,' with Circassian-like development of **/-š-w-/* > **/-χ^w-/*, **/māx^wə-la/* time-instr. > Goth. *mēl* 'day,' **/māx^wə-la/* time-gen. > Lat. *mētiōr* 'to measure out,'
c) PNWC **/məšə/* > PA-A **/mšə/* 'day,' **/məša-w/* time-predicative = 'day' > Kab. */maaχ^wa/*, Ub. */māχ^wá/* id. ('mās^wá').

- (66) 'sour, caustic liquid'

- a) Pontic **/saχu/* 'sour, caustic liquid,'
b) PIE **/səx^wə-r/* > Hitt. *šeħur* 'urine,' OIce. *saurr* 'semen,' 'impurity,' 'filth' *söggr*, *súrr* 'sour,' OE. *sēaw* 'juice, liquid,' Gk. *ὄει*, Toch. B. *sūwam* 'it rains,'
c) PNWC **/saχu/* > Kab. */saχ^wə/* 'lime, quicklime.'

- (67) 'people'

- a) Pontic **/rə-ğa/* 3rd, impersonal-collective,
b) PIE **/a-rəğa/* the-people > **/haryo-/* > Hitt. *arwa-* 'free man' (< **arya-wa-*), Ind.-Iran. **arya-* 'Aryan,' Gk. *ἄριστ-*, Runic *arjositiz*, Welsh *irr* 'charioteer,' OIr. *Airem* 'a god' ('guardian of the Aryans') < **aryaman-*,
c) PNWC **/(a)rəğa/* > Circ. */adəğa/*, Ub. */a:-dəğa/* 'Circassians,' Abz. */-rɣa/* 'people.'

- (68) 'house, family'

- a) Pontic **/ğuna/* 'house,'
b) PIE (**/ğuna-t^ha-q^ha/* >) **/wuna-t^h-q^ha/* house-of-belong > Gk. (Dor.) *Ἰάναξ*, *Ἰάνακτι* 'lord' (i.e., 'head of the family'), Toch. A *nātāk*, Phrygian *Ἰάνακτει* id.; PIE (**/ğuna-q^ha-ya-xa/* >) **/wuna-q^h-yaħ/* > Gk. (Dor.) *Ἰάνασσα* 'lady,' Toch. A *nāsi* id.,
c) PNWC **/ğuna/* > PCirc. **/wəna/* 'house,' Abz. */ɣ^wna/*; **/ğuna-t^ha/* > PA-A **/ğ^wəna-ta-ğ^wa/* house-gen.-person = 'family' > Abz. */ɣ^wnaɣ^wa/*.

(69) 'man'

- a) Pontic */wə-ġə-/ male class marker-man = 'man,'
 b) PIE (*wə-ġə-/ >) */wey-/ > Lat. *vīr*, Ir. *fēr*, Goth. *wair*, Lith. *vyras* 'man,' Skt. *váyas-* 'strength,'
 c) PNWC */wə-ġə-/ > PC */ġ^wə/ > WCirc. /(š^hə-)wə/ '(horse-)man,' Ub. /wə(də)/ 'devil'; */wə-ġə-/ > PA-A */ġ^wə/ > */ġ^wə/ > */ġ^wə/ > Abz. /aġ^wə/ 'man,' /-ġ^w/ 'agent'; */wə-ġə-a/ > Ub. /-ġ^wá/ sfx. on pronouns.

(70) 'giant'

- a) Pontic */yən-ra/ gigantic-gerund = 'the one who is big,'
 b) PIE */yən-ra/ > Skt. *Indra-* (hero of the Rig Veda), Av. *indra-* 'a demon,' Hitt. *innara-* 'a goddess' (odd semantics of the PIE term are explained by Pontic),
 c) PNWC */yən(-ra)/ > Circ. /yənə/ 'big,' /yənə-ž/ big-evil = 'giant,' Abx. /a-ynar/ 'the giant.'

(71) 'to say'

- a) Pontic */(-wə-)q'a-/ -(valence-)say- = 'to say' ('to talk'),
 b) PIE (*wə-q'a-/ >) */wəq'-q^ha-/ -talk-belonging-(?) > */?əw-q^hw-/ > Av. *aok-* 'to speak'; */?wə-q^hw-/ > Ved. *vī-vak-ti*, *Vakṣ*, Lat. *vōx*, Umb. *vepurus*, Gk. (F)έπος,
 c) PNWC */(-wə-)q'a-/ > WCirc. /-?^wa-/ , Kab. /-?a-/ , Ub. /-q'a-/ , Abx.-Abz. /-h^wa-/ 'to say.'

(72) 'mouth'

- a) Pontic */ġə-č^ha-/ edge-mouth = 'lips,' 'mouth opening,'
 b) PIE (*a-wə-ġə-č^ha-/ the-male-edge-mouth >) */haġ^w-s-/ , */hawġ-s-/ > Hitt. *aiš*, (obl.) *išša-*, Luw. *āš*, Lat. *ōs*, *aus-*, Skt. *ās-*, *óṣṭha-*,
 c) PNWC */wə-ġə/ > PC */?^wə/ 'mouth,' 'lips,' 'edge'; */ġə-č^ha/ > P-Ub. */ġča/ > */č^ha/ > /(fa-)č^ha/ '(nose-)mouth' = 'face'; */ġə-č^ha/ > PA-A */ġčə/ > */č^hə/ > Abx. /(a-)č^hə/ '(the-)mouth,' */yə-ġə-č^ha/ > P-Ub. */ġč^ha/ > */č^ha/ > /č^ha/ 'mouth.'

(73) 'cattle'

- a) Pontic */wə-ġə-(wə-yə-)/ male-cow/cattle-(being-one of-) = 'a grazing animal,'
 b) PIE (*wə-ġə-(wə-yə-)/ >) */?^wəw-y-/ > Hitt. *hawiš* 'sheep,' Luw. *hawi-*, Hier. Luw. *hawis*, Lat. *ovis*, E. *ewe*, Arm. *hoviw* 'shepherd,'
 c) PNWC */wə-ġə/ > Circ. /?^wəsə/ 'food, feed'; */wə-ġə-a/ > Circ. /?^wa/ 'cattle pen.'

(74) 'to be,' 'to be well'

- a) Pontic */?əčə-/ 'to be,'
 b) PIE (*?əčə-/ >) */?əs-/ 'to be' > Skt. *ás-ti*, Lat. *est*, Goth. *ist*; */?s-əw/ be-adv. = 'good, well' > Gk. *εὖ-*, Skt. *su-* (with lengthening of preceding vowels),
 c) PNWC */?əčə-/ > */č^ha/ > Ub. /ča/ 'good.' by influence of the preverb form; */-?əčə-wə-/ > P-Ub. */-č^hə-wə-/ >

/-š^wa-q'a-/ -good-say- = 'to speak well of someone';
 */?əčə-wə-/ > PC */č^hə-wə-/ > WCirc. /š^wə/ 'good,' Kab. /fə/ id.

(75) 'two'

- a) Pontic */t'q'o/ 'two,'
 b) PIE (*t'q'o/ >) */t'q^wə/ , */t'q^wə/ > */dwō/ , */do/ (with leveling to */dwo/) > Skt. *dvā*, *dvau*, OCS. *duva*, Gk. *δύω*, *δύο*, E. *two*,
 c) PNWC */t'q'o/ > PC */t'q^wə/ , P-Ub. */t'q^wə/ > /t'q^wa/ orig. 'twice,' PA-A */t'q^wə/ > Abz. /-q^w/ , Bzyb Abx. /-y^w/.

(76) 'six'

- a) Pontic */(w-)səχcə/ (masc. class marker-)six,
 b) PIE (*w-səχcə/ >) */s^wəχ^hs/ > Gk. *ἑξ* (< */swəχ^hs/), Lat. *sex*, Goth. *saihs* (both < */səχ^hs/), Arm. *veç* (< */wəχ^hs/), OPruss. *uschts* 'sixth' (< */wk^hs-t^ho-/), Av. *xšvaš* (< */švaš [cf. *xšnaiti* < */zīna:ti 'he knows,' Gk. *γνώτι*, E. *know*], but perhaps by metathesis < */šwaxš < */sweks/),
 c) PNWC */(s)əχcə/ > PA-A */xcə/ > Abz. /c-/ , PC */χcə/ > */χsə/ > Circ. /χə/; */(w-)səχcə/ > P-Ub. */x^wc^wə/ > */š^wc^wə/ > Ub. /fə/ , PA-A */x^wc^wə/ > */š^wc^wə/ > Abx. /f-/.

(77) '(hard) metal'

- a) Pontic */(w-)γə-(č'a)/ '(grammatical class marker [I]-)metal-(hard),'
 b) PIE (*a-γ^wc^wa/ > */hawc^wa/ >) */q₄awso-/ > Lat. *aurum* 'gold,' (*a-γ^wc^wa/ > */hawc^wa/ >) */q₄e₂so-/ > Lat. *ōrum* id.,
 c) PNWC */γ^wə-(č'a)/ > Bzh. WCirc. /γ^wə-č'a/ hard-metal = 'iron,' /γ^wa-a-p^ha/ metal-conn.-red = 'copper,' Ub. /wəc^wá/ 'iron,' Abz. /ġ^wa-(t'a)/ 'copper.'

(78) 'metal (object)'

- a) Pontic */yəχa/ 'metal (object),'
 b) PIE (*a-yχa/ > */hayχa/ > */hayš^ya/ >) */q₄ay-so-/ , */q₄y-es-/ > Lat. *aes*, Skt. *áyas-* 'metal,' Av. *ayah-* 'metal object,' Goth. *aiz* 'metal, money,'
 c) PNWC */a-yəχa/ > Abx. /a-ayxa/ , Abz. /ayxa/ 'iron,' 'metal.'

(79) 'son, child, foster child'

- a) Pontic */pa/ 'son, child, foster child,'
 b) PIE */pa-w-/ > Gk. **πάτριος* > *παῖς* 'child,' *παῖρος* 'little,' Lat. *puer* 'boy,' Skt. *putra-* 'son,' Osc. *puklúm*, Paelignian *puclouis*, Goth. *fawai* 'few,'
 c) PNWC */pa-w-ġə-/ > PC */-pa?^wə-/ > Bzh. WCirc. /-p^h?^wə-/ 'to rear'; */pa-w-ġə-/ > PC */-pa?^wə-/ > Bzh. WCirc. /p^h?^wa/ 'foster child'; */pa-y-ġə-/ > Ub.

(northerly) /-p'q'y-/ 'to rear'; */pa-ŋə-/ > Ub. (southerly) */paŋə/ > */pŋə/ > /(q̄^wa)p̄xə/ 'foster child'; */pa-ŋə/ > PA-A */px-ə/ > Bzyb Abx. /(a-)x̄^wp̄hə/ 'foster child.'

(80) 'son,' 'nephew'

- a) Pontic */(nə)pa-(t^h-)/ (lower-)son-(being/standing) = 'nephew,'
 b) PIE */nəpat^h-/ > Lat. *nepōs*, Romanian *nepot*, Ir. *niae*, OE. *nefa*, OHG. *nevo*,
 c) PNWC */pa/ 'son.'

(81) 'to sit (down)'

- a) Pontic */(ʔa)sə-(t'a-)/ (change of state-)sit-(down),
 b) PIE */ʔəs-/ > Gk. ἥ-μαι, ἥσ-ται, Hitt. *e-š-zi*, Skt. *āste*; */ʔs-ət'-/ > Lat. *sedēre*, Ir. *saidim*, Lith. *sėdėti*, Skt. *sad-*, Goth. *sitan*,
 c) PNWC */(ʔa)sə-(t'a-)/ > Bzh. WCirc. /-qa-sə-ta-/ -change of state-sit-down- = 'to sit down' (with deglottalization of affixes), Ub. /-s-/ 'to sit,' 'to be situated' as in /a-s-q'a-y-ā-s/ it-my-hand-dir.-dat.-sit = 'it is in my hand' (Vogt 1963:167, §1457), /-t'^wa-s-/ -down-sit- = 'to sit (down)' (with preposing of affix).

(82) 'to lie down,' 'to fall down'

- a) Pontic */-ʔə-(g-y-)/ -lie-(on-dat.-) = (1) 'to lie on,' (2) 'to fall on,'
 b) PIE */ləg^y-/ > Hitt. *laki* 'causes to fall,' (mid.) *lagāri* 'falls,' Gk. λέξ-ομαι, (Hom.) λέκ-το, Lat. *lectus* 'bed,' Ir. *laigim*, Goth. *ligan*, OCS. *ležati*,
 c) PNWC */-ʔə-/ > PC */-ʔə-/ 'to lie, to be prone' > Bzh. WCirc. /s-a-ʔ/ I-press-lie = 'I am lying down,' for */-g^yə-/ , note Ub. /-g^yə-/ 'on' (preV); */-ʔə-a-/ -fall-dat.- > PC */-ʔə-a-/ > Bzh. WCirc. /s-y-a-ʔa-a-y/ I-dir.-dat.-fall-th. v-past = 'I fell down'; with the same split in meaning as seen in PIE.

(83) 'sister'⁶

- a) Pontic */(w-)śəmc'a/ (class(I)-)woman,
 b) PIE */swəśs-ar/ woman-kin afx. = 'sister' > Skt. *svasar-*, Lar. *soror*, Ir. *siur*, Goth. *swistar*, OCS. *sestra*,
 c) PNWC */(w-)śəmc'a/ > Ub. /s^wəmc'a/ 'woman,' Bzyb Abx. /(h^w)śša/, WCirc. /ś^wəz/, /psaśa/ 'girl' < */p-śəmc'a/ child-woman.

CONCLUSIONS: First, PIE and PNWC are remotely related at a time depth of roughly 10,000 years.

Second, the sound system for the parent, Proto-Pontic, is likely that in (84).

(84) Proto-Pontic

p ^h	p	b	-	m	w
t ^h	t	d	t'	n	r
c ^h	c	ʒ	c'	s	z

č ^h	č	ž	č'	š	ž	y
χ ^h	χ	λ	χ'			
k ^h	k	g	k'	ḡ	ḡ	
q ^h	q	-	q'	x	ḡ	
				ḡ	ḡ	
			ʔ	h		
i		u				
e	ə,	o				
	a					

More work will have to be done to confirm all the vowels. The voiceless unaspirated series of stops is motivated by PNWC and seems to have fallen in with the voiceless aspirated stops in PIE. It is possible that this early loss led to later shifts and renewals in the source features of the voiceless stops in the various branches of Indo-European. Much more work is needed to trace out more complex sound laws. For example, there are some sets where a labial-lateral cluster in NWC seems to correspond to a labiovelar in PIE, such as Circ. /p'χ'ə/, Ub. /p'χ'a/, A-A /pš^y/ all 'four' (which behaves as though it were a single segment in A-A, violating as it does the PA-A cluster rule *C₁C₂ > C₂), compared with PIE *k^wetwer (*k^{hw}ət^{hw}wər/ or *k^{hw}ət^{hw}ər/) 'four.' It would seem from this vantage point that PIE was a gross simplification of Proto-Pontic. The history of the velar, uvular, pharyngeal, and laryngeal spirants, and /ʔ/ has already been delineated in (6)-(13). The affricates and spirants all seem to have fallen together into */s/, though further work is likely to show this to be an artifact of an overly simple image of PIE. The laterals seem all to have gone to */l/, though here too further work is likely to yield interesting results.

Third, with its grammatical class prefixes (Colarusso 1989a), Proto-Pontic looks very much like a Daghestan or Northeast Caucasian language, and, in fact, further work is bound to show that PIE shares a phyletic link with PNEC as well, probably through Proto-North Caucasian, and perhaps with Proto-Kartvelian as well (Harris 1990).

Fourth, despite its NEC-look, PIE was spoken contiguously to PNWC, with some forms of PIE sharing some isoglosses with the more northerly portion (Proto-Circassian) of PNWC.

Fifth, the PIE homeland was most likely along the northeast shore of the Black Sea, extending partially into the northwest region of the Caucasus, where its phyletic cousin dwelt. Proto-Pontic itself was likely to have been in the northwest Caucasus, extending up into what is now the Crimea and southern Ukraine. The steppe offered opportunities to exploit the horse in a nomadic economy, and this opportunity set the ancestors of Indo-European apart from their kinsman in the mountains and launched them upon the stage of history.

NOTES

- 1) The amateur archeologist, Geoffrey Bibby, suggested in 1961 that PIE was a Caucasian language that went

north and blended with a Finno-Ugrian tongue. This guess seems to owe more to the old notion that the Caucasus was the source for many of the peoples of Europe than it does to an informed notion of PIE, of any Caucasian languages, or of Finno-Ugrian. Friedrich's conjecture, therefore, takes historical precedent.

2) I use "Caucasic" rather than the more traditional "Caucasian" to avoid any naive confusion that somehow these are "white man's languages."

3) Given some of the recent publicity (Ross 1991, Wright 1991) surrounding the revival of the late nineteenth-century notion that every language is ultimately related to every other (Pedersen 1931:338-339), I wish explicitly to dissociate myself from any such efforts. In fact, most such notions try to link North Caucasian languages with those in Asia, such as Sino-Tibetan or Yeniseian, or even more remotely with the Amerindian Na-Dene, while linking PIE with Uralo-Yukaghir, South Caucasian (Kartvelian) or Elamo-Dravidian, and Afroasiatic (Ross 1991:138-139). The plausibility of what follows simply shows the folly of such grand lumping schemes.

4) There is one Northeast Caucasian language, the Richa dialect of Aghul, which actually contrasts these types of sounds (Kodzasov 1987). In the back of the mouth, it contrasts uvulars: pharyngealized uvulars: pharyngeals: adytals: laryngeals.

5) There are a number of resemblances between PIE and Proto-Kartvelian (Howard Aronson, personal communication; Alice Harris 1990; Gamkrelidze 1966 and 1967), so much so that an investigation similar to this one is warranted. Phyletic links between PIE and Proto-Kartvelian would, of course, establish PIE as an outlier of an ancient Proto-Caucasic.

6) Eric Hamp (personal communication) has suggested that the root here is merely */sar-/, with */swə-/ being the reflexive. His argument is based upon the Latin pair *soror* (< */swəsar-/) vs. *uxor* 'wife.' This has a parallel in Vajzë Albanian *r-ya-* woman-diminutive- = 'wife' vs. *var-ya-* sister-diminutive- with *v-ar-* (< */swəsar-/). If the Albanian form is not a parallel built upon Latin influence but rather derived from other Indo-European patterns, then it would suggest that the PIE was */swəsar-/ own-woman = 'sister,' */uk^h-sar-/ outer-woman = 'wife,' and this Pontic match would have to be rejected.

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The following letter was sent by John Colarusso to James P. Mallory on 21 June 1993. It is reproduced here with permission.

The question you posed about the northern border of the Caucasian cultures or peoples is a difficult one. Most of us feel that these peoples have pretty much been in place since earliest times, with substantial retreat on the southern rim and perhaps some internal shifting. A minority, Johanna Nichols, harbors a conviction that the Chechen-Ingush (or Nakh) peoples were once extensively spread to the north and east. I enclose a map from my book flier showing the oldest well

known northern border, which is only a few centuries old (the map had no date, but looked like an 18th century one). I should make a few comments, which I am sure you will find interesting if not a bit frustrating.

I know of no evidence for ethnonyms such as *nakh*, *nakhchuo*, or *veinakh* playing any role in the history of the steppe. I think that the Nakh peoples have come down out of the mountains in the last thousand years or so and were never out in the steppes to the north.

That the peoples who were further north, that is to say, part of the steppe culture, have found refuge in the North Caucasus is common knowledge — for example, the Ossetians who are not Alans, since they call themselves *iron*, not *alan* or *ilon*, but more likely Sarmatians since *sarma-tæ* is good Ossetian for ‘free-s,’ that is, ‘the free ones.’

Not common knowledge is the fact that the ethnonym *Kimmerioi* is continued in the Circassian tribal name /kʰʷəmər-gʷay/ with the second morpheme an ethnonym suffix. The name is now *Chemgwi* or Russian *Temirgoy*. So perhaps the Cimmerians were a Circassian tribe in the steppe who were displaced by the Scythians. Alternatively, they were an IE tribe who were Circassianized after being driven out by the Scythians. Since Herodotus says that the Cimmerians fought on foot while the Scythians fought on horseback, I would be inclined toward the first possibility.

The Circassian tribe /ʃʰəpsəɣ/ may mean ‘pointed head ones.’ Perhaps these are a Circassianized band of pointed hat Iranians or even some ancient Keltic group, such as the Bastarnae.

For more links, one should know that the Kingdom of Tanais takes its name from the Circassian word for the Don, /tʰaan/, which itself looks as though it may have given rise somehow to the Iranian word **dān*. *Tanais* would have been a straightforward Circassian toponym with a Greek *-s* on it, namely, **/tʰana-ya/ > */tʰan-ay/* (by rules still at work in Kabardian) ‘Tana-one of.’ Also, the Maeotic Sea is the Circassian name of the Sea of Azov, which is /mə-wətʰa/ ‘not (able) to dam up’ and refers to the narrow mouth of that body of water. I have also gazed upon the common Ukrainian onomastic suffix *-ko* and have wondered if it were not a Slavic version of the Circassian patronymic suffix */-qʷa/* ‘son of.’ As I understand it, there is no obvious Slavic source for this Ukrainian ending.

As to who was in the Ossetian area before the Ossetians, I do not know. I shall inquire among the scholars in Dzaujikau (Vladikavkaz) to see if they have any toponymic studies of their predecessors.

The question of the southern boundary of the Circassian peoples is germane to your question. Last June, I gave a paper in Maikop and had a roommate, one Jan Braun from Warsaw. He was giving a paper comparing Hattian with Circassian and Abkhazian. As luck would have it, I sat with him for two and a half hours one hot afternoon and went over all his forms with him. To my amazement, I found that he had a good case, with one solid cognate after another. The idea that Hattian is an outlier of NWC is not new, but I had never taken it seriously before. Perhaps the following scenario took place in the first few millennia BC.

South Caucasian (SC) extended further south than it does today. North East Caucasian (NEC) also had some southern outliers, perhaps cognate with Khinalug, which stands alone in contrast with all the other NEC languages, although it is clearly cognate. Fährnich at Jena has written a number of papers, none of them conclusive but all of them interesting, that Sumerian, Urartean, and Hurrian may be distantly related to Georgian or some of the Daghestani languages. I would agree with the general wisdom that the oldest traceable language(s) of the Middle East are (a) western outlier(s) of Elamite, since Ur is clearly cognate with Dravidian *urra* ‘village.’ I then see some intrusions from the southern Caucasus, which at that time would have been more likely Armenian highlands, northern rim of the ME. The NWC languages would have been in eastern Anatolia extending up along the Black Sea to the Caucasus massif. The NEC languages would have been confined to Daghestan and to what is now Azerbaijan. The early Semitic incursions into the ME would have had the effect of pushing the SC and NEC languages further up onto the south slopes of the Caucasus, stranding a few odd languages behind for us to puzzle over, the last surviving one being Caucasian Albanian (which should more accurately be called “Alvanian”), which was spoken until about a thousand years ago and is still spoken in the form of the NEC language Udi.

Perhaps too the early IE (Anatolian branch) invasions of Anatolia, coupled with pressure from the northerly movement of SC peoples, would have pushed the NWC peoples further north and across the massif, stranding Hattian in the process. The push of NWC peoples would have in its turn pushed PIE peoples (minus the older Hittite branch), who were the Kurgan culture of the northwest Caucasus zone, out into the steppes.

Thus, the oldest Caucasian language of the North Caucasus was PIE itself, phyletically cognate with PNWC (that is, it was that branch of PNWC that originally fell on the northern side of the massif and because of its isolation it was deviant from the rest of the family) and probably more distantly from Proto-NEC.

A tantalizing piece of evidence that PIE itself may have been in contact with some of the more southerly NEC languages is the deviant word for ‘horse’ in Udi, /ʔəkʷ/, where /ē/ is a pharyngealized vowel. This is clearly a loan into Udi and suggests an old PIE **/ʔək-w-/* with the initial laryngeal being a voiced pharyngeal. I got this personally from Wolfgang Schulze-Fürhoff at Munich, a noted specialist on southern NEC (Lezghian) languages. (Perhaps related to this matter is the odd fact that glottal stops among the Nakh branch of NEC are always pharyngealized, so perhaps the PIE was actually **/ʔʰək-w-/*.) This loan into Udi (Proto-Alvanian) could be explained if PIE occupied the northern plain and foothills of the Caucasus at a remote epoch so that some of them, much like the Southern Ossetians today, could have spilled over the massif and had some contacts with people in the south, especially toward the center of the Transcaucasus. This is precisely where Udi is found today and presumably where the ancestor Alvanian was based before its short-lived political expansion.

If you are more comfortable binding the Anatolian branch in with the rest of IE on a contemporaneous basis, then I am content to have the SC peoples pushing the NWC ones further north and to leave the Hittites out of this movement.

How's all this for speculation?! Somehow I feel that this model, however scantily clad in data it may be, has a certain allure about it and explains a good deal of the odd, archaic linguistic data.

THE PRE-CLASSICAL CIRCUM-MEDITERRANEAN WORLD: WHO SPOKE WHICH LANGUAGES?

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We attempt to clarify the cultural and linguistic relations in the areas around certain ancient sea-ways before the expansion of the Indo-European speaking Greeks and Romans on the coasts of what the latter called *Mare Nostrum*.

Our particular emerging linguistic picture of the Bronze and Early Iron Age peoples in the lands around the Mediterranean has developed through various conversations over many years while we were colleagues at Boston University, and since. We say "emerging" because the picture still has lacunae at important points which will probably be filled eventually, and we feel reasonably confident new information will confirm the basic outline presented here.

In this discussion, we consider the areas from the Middle East to the Iberian peninsula, which includes some peoples fairly inland from the shores of the Middle Sea, but for which there is evidence of maritime connections — e.g., Sargon, founder of the empire of Agade, 22nd century BC, extended his realm to the Mediterranean, half a millennium after Lugalraggizi, king of Uruk, had done the same, but Sargon may have reached Cyprus. Our focus is the Mediterranean, but the center of gravity from which developments are stimulated is often this eastern end. We also append limited extensions from our defined circum-Mediterranean up the north Atlantic coast, and down the Persian Gulf.

This effort to formulate our perspective was sparked by Hal's review article of Merritt Ruhlen's marvelous "Guide" book (reprinted in *Mother Tongue* 20).

Any effort to interpret data must be appropriate to the nature of that data; we agree that a complete (as possible) understanding of the past of human societies requires interdisciplinary melding of contributions from linguistics, archeology, and biology: there must always be an attempt to find the language once spoken in each archeological culture, and conversely to locate the sites where extinct languages, including each stage in their evolution, were spoken. Also, the genetic characteristics of the speakers of those languages, and

inhabitants of those sites, should be determined as closely as extant evidence allows. But evidence (in each field) is often disputable; that a datum is evidence is less often challenged than that the deduction made from it is sustainable. This is especially problematical where, as in this paper, scholars have to reach, sometimes uncomfortably, into several specializations for the necessary data. All this is complicated by the circumstance that each of our kinds of data are mutable: peoples learn new languages and sometimes give up the old, as the people of Cornwall over generations learned English, and in the eighteenth century Cornish became extinct; acculturation occurs (no culture is entirely *sui generis*); genes flow from one gene pool to others. Consequently, discussion of each proposed linkage of fact with fact as well as of facts with interpretations is urgently required from several scholars with varying specializations. That need, which is felt by all "long-rangers," is the *raison d'être* of *Mother Tongue*.

1. There is no mention in Hal's section on "Isolates" and "Unclassified" languages (*Mother Tongue* 20:19-22) of a number of ancient Mediterranean written languages for which there was more information available in 1987 than Hal realized, and more has since appeared. Let us review the written language situation.

In the 2nd millennium BC, diffusion of writing (which had begun much earlier) reached into new localities, and non-literate peoples intruded into literate areas and began writing new languages in old scripts, or sometimes a shift in writing systems occurred; these developments continued into the 1st millennium. The impulse came largely from the Levant, but also from Egypt. Graphological developments appeared around the Mediterranean; and a new linguistic element is discernible in the Persian Gulf at Dilmun. The relics of these writing systems, scant as they usually are, offer the possibility, with careful analysis (and luck) to identify old languages. Consider how mistaken our calculations of (pre)history might be without the documents of Mesopotamia, Egypt, and the Hittites; other scripts must contribute to our understanding of the outer reaches of this ancient *oikoumene*.

A list of old languages (@ indicates discussion to follow), most of which are not known to have left direct descendants, include:

Gutian, Kassitic, et al. of the Zagros region of west Iran : still unclassified.

Elamitic of southwest Iran and east Iraq.

@Dilmunic of the upper Persian Gulf; this is Hal's construction, which Dan accepts.

Hurrian, Urartean, Mitannian of eastern Anatolia, Armenia, and north Syria: basically one language of the Caucasian phylum. (If the Kurds ever get their country, it will approximate Hurrartean land.)

Hatti/Khatti of Anatolia : another Caucasian branch.

Hittite, Lydian, Luwian, Carian of Anatolia : archaic Indo-European.

Linear B of Crete: Mycenaean (Mykenean) or archaic Greek.

@Linear A or proper Minoan of Crete. (It is an oddity of the archeology of that area that the stratum on top is called B while that underneath is called A. Minoan is earlier than

Mycenaean, as everyone knows, but A and B get confusing!)

@Eteocretan, or non-Greek language in Greek script on Crete.

@Eteocyprite, ditto of Cyprus.

@Meroitic of northern Sudan : modern Nubian of the same area is not descended from Meroitic and perhaps not even related to it.

Libyan of various locales and times in the northern Sahara fringe : it probably has a direct descendant in Tuareg, which still preserves the ancient writing system. Berber of Afroasiatic.

@Iberian of ancient Iberia (the one in Spain and Portugal, not in the Caucasus).

@Tartessian or Tertessian or Southern Lusitanian : South-western Iberia

@Celtiberian, or Celtic of Iberia : Celtic of Indo-European.

Aquitanian of southwestern France ; probably early Basque.

Various poorly known specimens of early Indo-European in Italy and the Balkans, e.g., Venetic, Messapic, Illyrian, Thracian, et al. Illyrian may be ancestral to Albanian, an oft mentioned connection. Phrygian should also be listed here.

Etruscan of central and northern Italy : well-known problem.

@Various poorly known but historically mentioned specimens of later or Classical Greek in southern Italy, Sicily, Iberia, southern France (Marseilles, for example), and Libya.

@Sikani (Sicani) and/or Sikuli (Sicili) of Sicily : underlies Greek (for example in Syracuse, or Siraguz in modern Sicilian), Punic, and of course Latin. A shadowy but important matter. (May have been spoken in Calabria and thereabouts before Greek took over circa 500 BC.)

A couple of questions:

Where is Trojan? Has a written version of the language of Troy ever been found? How could Troy have been non-literate? It would be very interesting to have even a scrap of an inscription! In Homer, Greeks and Trojans talk to each other, but a poet need not mention interpreters or bi-lingual persons, yet possibly they could talk without intervening helpers: Greek traditions derive some royal genealogies from Troy (but — even if these have historical substance — incoming rulers don't always speak, on arrival, the subjects' language). Does Trojan belong to an Anatolian cluster with Hattic etc., or with Indo-European? The Aegean coastlands of Anatolia became Indo-European speaking with Lydian etc., while central Anatolia received Hittite, and Indo-European Phrygian subsequently followed into Asia Minor. We hold the usual view that these Indo-European languages were intrusive from north of the Black Sea, and the time of movement was about the same as the descent of the Mycenaeans' ancestors into the Balkans. Troy was in the path of Indo-European migrations into Asia Minor. Had it become Indo-European before the time of the Trojan War?

What was/were non-Carthaginian pre-Roman language(s) of Sardinia and Corsica? Were any of them written? And of Malta? Malta has always been a strategic island stronghold between the eastern and western half of the Mediterranean. Modern Maltese is a dialect of Arabic with Turkish loan-words for naval terms, but what underlies this?

The Bronze Age culture of the island was distinctive, not fitting easily into either the eastern or western Mediterranean culture areas of that time.

2. The sea is a road for diffusion but not necessarily of migration. Early migrations apparently did occur with the spread of food production. Cyprus received a pre-pottery Neolithic, probably from the nearby Fertile Crescent to the east, but possibly from Asia Minor to the north. In *Mother Tongue* 14, Hal suggested that Anatolia, as the source of the Mediterranean Neolithic, spread Dene-Caucasic languages into the Aegean islands, the Balkans, and thence westward through the Mediterranean. Four languages, Minoan (Linear A), old Sicilian, Iberian, and Tartessian, "without any proof at all" were suggested as Dene-Caucasic. The logic is that the people who spread farming settlements spoke some language(s) which they brought with them, and Anatolia at the time (*pace* Renfrew, Ivanov, Gamkrelidze & others) was Dene-Caucasic speaking.

Cyrus H. Gordon¹, eminent Semiticist, attaches Eteocyprite as well as Minoan to Northwest Semitic. We find Gordon's presentation persuasive. Hal gives examples of the lexicon of Minoan with clear Semitic affinities:

Minoan	Semitic	English
<i>pe</i>	<i>pe</i>	mouth
<i>-ti</i>	<i>-t</i>	feminine ending
<i>ya-ne</i>	<i>wyn/yn</i>	wine
<i>u</i>	<i>w</i>	and
<i>ku-ro</i>	<i>kull-</i>	all
<i>re</i>	<i>le</i>	to, for, as
<i>ra-re</i>	<i>leli-t</i>	night-demon; night (Amharic)
<i>kʀn</i>	<i>kʀn</i>	horn
<i>pi</i>	<i>bi / be</i>	in (Arabic/Amharic)

Gordon has built on Ventris' work, taking the sound values of Linear B and thereby finding the vocalization of Linear A. Gordon also had later inscriptions of Eteocretan in Greek letters; again the sound values were attributable to the non-Greek language. The two periods yielded the same answer, the pre-Greek language of Crete was Semitic! In epigraphy, one has to figure out what sounds are represented by what symbols (graphemes) and then what meanings are associated with the combinations of sounds (or sometimes a single sound). This is what both Ventris and Gordon did; why has Cyrus Gordon not become famous for this work?

Another facet of Gordon's discovery is that the Semites had not occupied Crete very long before the arrival of the warrior Mycenaeans. He believes the Semites built the great palaces of the Minoans, but they were not the earliest natives of Crete (p. 43). Those who derived from the Cretan Neolithic were a different people — who did not write, darn it! One implication of this would be that human genetic studies of Cretans and Cypriotes would not necessarily show much special evidence to link them to modern Lebanese or coastal Syrians. Ruling classes and elites are sometimes lost to history (as far as genetic traces are concerned). And while Hal's

hypothesis that the Minoans were originally Hattic or such like is demolished by Gordon's Semites, still the native population may yet (despite all the subsequent invasions?) show biological affinity to Turkey rather than to Greece or the Levant. At least archeology shows that the Cretan Neolithic was derived from Turkey.²

Gordon also discusses an intense interaction between Semitic of the Levant and archaic Greek taking place elsewhere in the Aegean. He believes that Levantine Semitic was dominant in the Nile Delta, rather than Egyptian, and that eventually the mysterious Sea People came out of this Greek-Semitic mix, a proposition that deserves to be more closely examined.

3. Later, in the 1st millennium BC, Semites from Phoenicia were trading, sometimes settling, in the Aegean, Sicily, southern Iberia, and north Africa. They did not write down any description of the languages they encountered in these places. The Aegean was already, or becoming, Greek-speaking, and north Africa Berber-speaking, but the islands of central and western Mediterranean are obscure. So is Iberia.

Classical authors call the pre-Hellenic inhabitants of Sicily by three names: Siculi, on the east, Sicani, in the center, and Elymi, on the western tip of the island. The first two names are variants of a single vocable, which would suggest that the two ethnicities were closely related and that their speech was perhaps dialects of the same language, if it were not that classical authors insist on their distinction. Archeology accepts a distinction, attributing Sicani to a stone age culture, which once held all the island, and Siculi to an intrusive Chalcolithic culture from mainland Italy which arrived c. 700 BC. Mycenaean contact, for which there is legendary and archeological evidence, would therefore have been with Sicani. Were the Sicani linguistic relatives of the pre-Minoan Cretans?

The Siculi were apparently Italic in speech; a leader had a title almost identical to that recently used, *duce*. The Siculi, who once lived on the peninsula across the Messinian strait, were relatives of the Messapii of Calabria, for whom we have about 50 inscriptions in the Tarentine-Ionic alphabet (c. 400-150 BC), and whom Herodotus said had been subjects of "King Minos."

In eastern and southern Iberia, writings occur. It looks like a case of Kroeber's "stimulus diffusion," rather like the Cherokee in the Carolinas and the Vai in West Africa. Since the Iberian writing is primarily *syllabic*, like Akkadian, Amharic, and the Linear A & B scripts, it might reflect very early pre-alphabetic Levantine writing or a re-adaptation of the Punic alphabet to writing syllables instead of single consonants. Some of the inscriptions were written in Greek letters, however, and thus were alphabetic. (The matter of these writing systems used by Iberians is more complicated than our statement indicates, but since knowledge of language rather than writing system is our goal, we end that discussion here.) In any case, a corpus of words and bound forms has been acquired by much scholarly work, but formerly was insufficient for classification.

James M. Anderson (University of Calgary), in a new book,³ has done a remarkably clear and sophisticated job of presenting his theory. Jürgen Untermann, a senior colleague at

Cologne, who may have produced by now a monumental opus, was helpful to Anderson during his research, so there is support for some of his interpretations. Anderson clearly defers to Untermann as an authority on Iberian.

Anderson found, though he argues quite diffidently, that *Iberian is related to Basque*. It is not the ancestor of the Basque dialects but related to them as a cousin. There are a number of systematic differences which strongly suggest a clean separation between Basque and Iberian. There are also systematic sound correspondences (which should reassure our shorter rangers).

(Symbols /S/ and /R/ represent sounds with dots under them.)

Iberian	Basque	English
<i>gais-kar</i>	<i>gais-gaRi</i>	harmful
<i>gaiS-kata</i>	<i>gais-kata</i>	wound
<i>gais-egin</i>	<i>gaitS-egin</i>	do evil
<i>gaiS-esa</i>	<i>gaitS-etSa</i>	misanthrope

Generally, morphological comparisons are hard to make because the grammar of Iberian is not well represented in the inscriptions, although phonological similarities are noted by Anderson. Here are more examples of sound correspondences:

Iberian	Basque	English
<i>ausa</i>	<i>autS</i>	ashes
<i>arse</i>	<i>arts</i>	bear (animal)
<i>akel</i>	<i>aker</i>	billy-goat
<i>aS</i>	<i>atS</i>	breath
<i>bel / beles</i>	<i>belt</i>	black, dark
	<i>belex (beleS)</i>	black, dark (Aquitania)
<i>bioS</i>	<i>biots</i>	heart
<i>borste</i>	<i>borts</i>	5
	<i>bost</i>	5 (West Basque)
	<i>borsei</i>	5 (Aquitania)
<i>ilS</i>	<i>iltse</i>	to die
<i>sorse-</i>	<i>sortsi</i>	8
<i>unsir</i>	<i>untsi</i>	boat
	<i>untsiratu</i>	to embark

There also appear to be dialect differences within Iberian, roughly southern versus eastern. The writing systems vary from north to south due to model stimuli, respectively of Greek and Punic.

By contrast, an earlier book (1979), which Hal overlooked in 1987, was a report on an international conference on Iberian held at Tübingen.⁴ Erudite, cautious, but no decisions, although one author did examine the question: "Is Iberian a Berber language?" and found the Berber connection to be a few borrowings but otherwise very weak. There were some cautious suggestions of links with Basque but some also for links with Semitic. These two books between them have many references to a highly exotic literature; one which Anderson appears to have summed up adequately.

Both Anderson and the conference report review much archeological data on Tartessos, west of the mouth of the

Guadalquivir river, but hardly anything on grammatical or lexical substance of the Tartessian language. It seems not to be Iberian. Tantalising suggestions of Etruscan and Hurrian connections are found but much of that relates to the peculiarities of the writing system. Tartessian is most likely to turn out to be related to Iberian, Basque, Berber, or Punic, which are found in proximity.

Instead of coining a term like Basque-Iberian, we propose that the family of Euskera and Iberian be called Basque. Euskera can be that or Northern Basque, while Iberian could be Southern Basque.⁵ It would be easier to think of it as a small phylum than as an isolated language.

In 1821, Alexander Humboldt was apparently the first to assert that Iberian was related to Basque. His hypothesis was denigrated until recently; now he should receive due credit. Such is the way with good hypotheses which lack sufficient support and are too early for people to accept. Yet it is clear from Anderson's comments that many scholars over the next century and a half agreed with Humboldt. Perhaps the theory that Iberian is related to Basque should be thought of as the traditional one.

By the time Iberians marched with Hannibal against the Romans, they had lost their hold on much of their country, having lost it to the Celtiberians, but they still held the "best" territory on the east, i.e. Valencia, which North European vacationers used to call the Gold Coast.

4. Who were the Celtiberians? In language, just plain old Kelts but in body and genes probably seriously mixed with Basques or Basque-like peoples. One expert sternly criticized the common belief that Celtiberian was Celtic, maintaining rather that it was a very early or first wave of Indo-European coming in from the east. Another colleague giggled and pointed out that such was an accurate picture of Celtic itself, wasn't it? More arresting was the notion that Celtiberian was the source of the "Q" Celts of the British Isles. There is a considerable distance linguistically, but not geographically, between the "P" and "Q" Celts. The "P" Celts are the Welsh, ancient Britains, Cornishmen on the larger of the British Isles, and the Breton of France; the "Q" Celts are the Irish and those Scots who are not Picts. Pictish is not well documented but is probably Celtic.

The "Q" Celts may have sailed from Spain to Ireland, which may be surprising considering that the Celts had come to Iberia from the interior of the continent (presumably lacking maritime skills), but co-opting the experienced Iberians would give them what they needed to navigate at sea. Compare the map on page 61 of Professor Stuart Piggott's *Ancient Europe*. A pre-Celtic connection of the Atlantic coastal region of Iberia and Ireland is indicated as far back as the period of megalithic chambered tombs (the distribution of which is more extensive than just these two regions, but — significantly? — the North Sea coast of England is not part of this culture-area). (Incidentally, the distribution on Piggott's map is precisely what Humboldt found to be the distribution of place names in Atlantic Europe that seem to be explicable in Basque, or perhaps a related tongue.) Thus one might expect Iberians to bring the Celts to Ireland rather than to England. There is an Irish tradition that a prince of Heber, or Eber, came from Spain with his followers. Some Irish writers have suggested that Eber

= Ebro (the country of the river which rises in northwestern Spain), and that the time may have been when the Romans were gaining control of the northwest of the peninsula, which would make this rather late, but this prince need not be the first to migrate, rather the last to leave when the war was lost, or nearly so.

It is interesting that finds of Ogham script center in the south of Ireland. It is also claimed that the order of the alphabet does not follow the Latin order, which Western European languages have taken over, but one of the early Greek alphabets, which might have been learned in Ebro country (but also of course it might have been from Marsalla [Marseilles]).

There are "similarities" of Irish to Basque which keep getting mentioned. Hal dispatched a student to check on that some years ago; the answer was yes, there are quite a few. More precisely, it looked like Irish had borrowed some words from Basque. There were, and are, Basques in France, and the contact might have been there, but in view of Celts in France and England, it seems more likely that the Celts with Basque loan-words came from Spain.

The blood groups are consistent with an Irish-Celtiberian connection. In Rhesus and ABO, the Basques and peoples of the so-called "Atlantic fringe" are alike, and most of the latter are presently, or were formerly, speakers of Celtic: Scots, Irish, Welsh, Breton, and a few western districts of France south of Brittany. Ordinary Spanish, English, and French persons do not have as much Rhesus negative (haplotype "r") and "O" (of the ABO system) or so little "B," but they share the tendency. Some parts of highland Scotland have almost as much "r" as Basques do. For a real surprise, contrast the Sardinians and the Basques; in Rh, ABO and MNS, they differ quite smartly, even though Sardinia is much closer to the Pyrenees than is Aberdeen or Dublin. Berbers, Basques, and Celts, in contrast to their neighbors, tend toward more "N".

The presence of Basques in France when the Celts arrived from the east might be sufficient to explain the serological peculiarities without any theories of Celtiberian movements. (A lot depends on the proportion of intruders and autochthones in the mixed population that eventually spoke some form of Celtic.) There is plenty of evidence of Basques in southern and central France during Celtic times. The question is how far north they extended, and whether or not they can be identified with the Bell Beaker people?

5. The last problem of the western Mediterranean which deserves more attention than was given it is the question of possible relationship of Basque to Berber.

Point One. Precisely the same biogenetic traits which link Scotland and the Pyrenees also link the Pyrenees and the Atlas mountains where Berbers predominate. As Mourant⁶ and his colleagues pointed out some years ago, this pattern is definitely not shared by the lowland Arabs of the Maghreb. Though they tend, like the Bedouin of Arabia, toward high percentages of "O," they are not high in "r" and have high amounts of "M," while Mourant found that the cline from predominant "M" to very high "N" ran right up the mountains from the Arabs to the Atlas Berbers. (Very high "N" does not occur again in the Old World until one reaches highland New Guinea!)

Point Two. Hans Mukarovsky was very impressed with the lexical evidence of Berber and Basque connections, as has been several times mentioned in *Mother Tongue*. Some others have agreed with him. Recently, Dolgopolsky came to believe that Basque is probably an extension of Afroasiatic. Recall that in *Mother Tongue* 20 Blazhek thought that Elamitic was related to Afroasiatic. (Afroasiatic may have wider extensions east and west than previously believed!) So the Basque-Berber connection is a serious competitor to the proposed link of Basque to Caucasian. If we ever get good blood group data on Caucasian peoples, it will help a great deal. We hope Cavalli-Sforza's new book has such.

Point Three. It has been proposed that the Maghreb got its Neolithic by sea from the east Mediterranean in the same basic "stream" that brought the Neolithic to Iberia. That is more credible than a more difficult land route, at least for migrating farmers, from Egypt to Cyrenaica and then across the northern Sahara to the Maghreb. However, we cannot settle a question beyond our competence; archeologists will eventually tell us more conclusively whether it was from Turkey by sea or from Egypt by land.

Point Four. Everything alluded to above is germane to the arrival of the Berbers from the theoretical Afroasiatic homeland. There are no claims that we know about which locate that debated area in the Maghreb. Thus the Berbers are seen as immigrants from elsewhere, and it is easy to see the putative movement of the Neolithic from Egypt to the Maghreb as really a march of Berber farmers. But maybe the place was already occupied by old Basques? (There is a continuity of a Mesolithic culture across the Strait of Gibraltar.)

As Hal has argued in the Borean hypothesis (*Mother Tongue* 14), the Maghreb is one of those very old areas of human — modern human — habitation, along with east Africa, Ethiopia, and the Levant. One could expect an autochthonous phylum in such a place.

6. Just down the Nile from ancient Egyptian, the Meroeans with their language, Meroitic, have remained a mystery. Most of the problem was a paucity of vocabulary and clear morphology to use in classifying. Written in Egyptian characters, but lacking a Rosetta Stone, Meroitic has had to be pried loose from its context word by word. Consequently, most taxonomically inclined scholars have not tried to classify it. However, recently a lexicon has been published by Dmitri Meeks, an Egyptologist.⁷ We have not seen it yet. Using that lexicon, M. L. Bender⁸ concluded that Meroitic was a member of East Sudanic or at least of the larger phylum Nilo-Saharan (N-S). This is a conclusion that Bruce Trigger had reached more than twenty years ago.

However, if more data are now available, in effect a secret until now, then the rest of us ought to "have a go at it" too. This is particularly true because there is a Russian view which entirely disagrees with Trigger and Bender. Theirs is the opinion that Meroitic is Afroasiatic (A-A). Given its location as contiguous to Egyptian (Afroasiatic) and nearness to the Nilo-Saharan heartland just to the south in the Sudan plus the presence of Nubian (properly Nile Nubian) of Nilo-Saharan as its successor plus the nearness of Beja (Afroasiatic) to the east, Meroitic could easily be related to *either* phylum. It is also

relevant that Nile Nubian itself, while most assuredly a Nilo-Saharan language at base, is often called "Hamitic." At a minimum, it appears to have a fair amount of similarity in its vocabulary to Cushitic languages (Agau of north Ethiopia) as well as Beja and Ethiopic (Semitic). These have sometimes been attributed to Ethiopian conquests of Meroe and Nubia, as well as the sharing of religious customs and/or priests due to their common Christianity. (Nubians have been Moslem since only a while before Columbus sailed west.) It is, naturally, possible that Cushitic similarities are *cognates* in Meroitic, yet borrowings in Nubian. The whole business needs thorough re-examination.

In looking at Bender's statement of putative cognates between Meroitic and Nilo-Saharan — quite a reasonable presentation —, Hal noticed at least three basic vocabulary items which have solid bases in Afroasiatic. We cite one here: /ab/ which means 'mouth.' It occurs in three sub-phyla of Afroasiatic, even in Omotic. It was replaced in Egyptian and Berber but arguably survives in Chad — it goes back easily to Proto-Afroasiatic. And in the neighboring Bilen of Agau, it is in fact /ab/. Borrowing of this conservative morpheme is (so far as we know) unheard of in Afroasiatic.

Thus, we surmise that Meroitic should go back into the "Unclassified" category, but for the reason that it now has some data for scholars to quarrel over.

We cannot help wondering what biogenetic heuristics there are to help us with Meroitic. If our colleague Egyptologists can advise us how the ancient Egyptians classified the Meroeans — white, red, brown, black, or whatever —, that might help.⁹ There is no doubt that the Nile Nubians and many other "northern Sudanese" are akin to the Tuareg (Afroasiatic), the Teda (Nilo-Saharan), the Fulani (Niger-Congo), and north Ethiopians (Afroasiatic) in blood groups. That fact does not predict the linguistic affiliation very well, as we can see by the list. Moreover, the emphatically Nilo-Saharan Kunama and Nara (Barea) of Eritrea clearly show considerable gene flow from the Cushites so that in this part of Earth one has to prove a linguistic link with a biogenetic group, not merely assume that one begets the other.

7. The fabled land of *Dilmun* lying at the interface of Arabia, South Asia, and Mesopotamia seems to have had its own language. Information on that place has been growing apace due mostly to the efforts of Danish archeologists but under the inspired leadership of Geoffrey Bibby. Their efforts and those of others embrace the island of Bahrain, Qatar, United Arab Emirates, Oman, and adjacent parts of Saudi Arabia. While the prehistory of the time we are focused on shows a great deal of interaction with Mesopotamia, Iran, and the Indus valley — including periods of political dominance by some of them and much use of their writing systems —, still it is now clear that there was a native element present, and one scholar has declared that there is evidence of a distinctive language.

F. Højlund reported¹⁰ that an impression of mixing of populations in Dilmun was created by different episodes of conquest or of heavy trade by the outsiders. Nevertheless there was a clearly visible native or original population, associated with "Barbar" culture, which showed up in the late 4th

millennium BC on the eastern shores of Arabia and by 2150 BC on Bahrain. "It was a people with a distinct culture with a strictly local burial custom indigenous to the eastern shores of Arabia." A century later on Bahrain, "one can trace a marked influence from the Indus valley in the archeological material. How to interpret this influence is a matter for further investigation, but, clearly, immigration of Indus traders to Bahrain is not the only possibility. Around 2000 BC, the influence from the Indus valley is replaced by a North Western influence, which may be related to an immigration of Amorites, which was paralleled in South Mesopotamia. Around the middle of the 2nd millennium, when Dilmun was made into a Mesopotamian province, we are on firmer grounds with respect to immigration of foreign people." Oddly enough, that is the period of the Kassite conquest of southern Mesopotamia and Dilmun. The Kassites remained for many centuries.

In regard to language, Højlund tells us: "The fact that the Dilmunites adopted seals in the form of stamp seals as in the Indus Valley, instead of cylinder seals as used in Mesopotamia is equally significant, though one should note that round stamp seals are in this period found not only in the Indus Valley, but occur at several places in the Indo-Iranian area, such as for instance Anshan. Eight out of 9 seals dating to (this period — HF) at Qala'at al-Bahrain belong to the Arabian Gulf type. There are also triangular seals of local type. Then he called in an expert on Harappan seals and: "According to Parpola, who has studied the Indus inscriptions on Arabian Gulf seals, the specific sign combinations are in most cases never attested in the Indus Valley... This means that the language is probably not Indus, but some other language spoken in the Near East, such as the language of the Dilmunites. These seals have been interpreted by Potts ... and others as seals belonging to Indus traders settled in Dilmun, but it is perhaps equally likely that they belong to Dilmunites who have taken over the Indus script to write their own Dilmunite names."

If we all hang there patiently, hoping that the research speeds up or we live long enough, we may yet find out who those people were and what kinship they have to Sumerians, Elamites, and Semites! There is skeletal material — a lot of it — which may help a bit. Perhaps it is Sumerian, i.e., it may be relevant that Paul Zimansky has located the Ur-Sumerian spot in southern Iraq (at Larsa), which is not terribly far from Dilmun.

POSTSCRIPT

As our trip¹¹ around the Mediterranean brought us back to Sumeria, so have recent publications. Writing in the NY Times on 9 November 1993 (p. B7 and B9), John Noble Wilford reports on some serious progress in our discovery of that remarkable 4th-3rd millennium BC in Mesopotamia. It seems that the urban revolution was more widespread than thought and that larger cities occurred in Syria and southern Turkey than had been anticipated. In effect, we have discovered a very clear route of diffusion of urban civilization and Sumerian cuneiform writing from the lower Tigris to the upper branches of the Euphrates and to the Mediterranean coast (Ebla). Except for the fact that Semitic names were being written in Sumerian characters, one might suppose a migration

of Sumerians from south Iraq to Kurdistan to coastal Syria. The dates of the new finds hover around 2500 BC, making them the contemporaries of both the Akkadians and the early Northwest Semites of Ebla.

The linguistic data underneath this exciting new epigraphy have not been revealed yet, as far as Wilford reported. Let us hope that the new data are not "screwed up" as that of Ebla was said to have been. One important question is whether the Semitic involved is closer to the Northwestern branch (Eblaite, Ugaritic, Canaanite, Hebrew, etc.) or to the Eastern branch (Akkadian, Babylonian, Assyrian). Since the dates are only 500 years or so after some estimated dates for Proto-Semitic, it is possible that another branch of Semitic may be found; it would not be terribly different from Akkadian or Eblaite. It is also likely that 3000 BC is probably not old enough to accommodate the diversity in Semitic. A final question, of course, would be whether anything the likes of Hurrian or Urartean or Kassitic is revealed by the new data.

For those who wish to pursue these finds, the names of the cities and the archeologists associated with them are as follows:

1. *Tell Beidar*: Up the Khabur River in far northeastern Syria. Dr. Marc Lebeau of the European Center for Upper Mesopotamian Studies. In Brussels. He led a team from six countries, to wit, Belgium, France, Germany, the Netherlands, Spain, and Syria. So you can all guess who to ask in your own countries. Dr. Harvey Weiss of Yale University recently visited Tell Beidar and might be willing to tell American colleagues all about it. On Wilford's map, but not in the text, is *Tell Birak*, which is perhaps 60 kilometers or so northeast of Tell Beidar.
2. *Kazane Hoyuk*: Up the Balikh River in extreme southeastern Turkey between the towns of Urfa and Harran. They propose that it correlates with the ancient city of *Urshu*, mentioned in Sumerian texts. Dr. Patricia Wattenmaker of the University of Virginia was director of the excavations at Kazane Hoyuk. Dr. Glenn Schwartz of Johns Hopkins University is said to be familiar with the site. Archeologists from the University of Chicago were also involved, probably from the Oriental Institute.
3. *Tell Leilan*: In "the fertile plains of Syria near the borders of Turkey and Iraq." Extreme northeastern Syria, may be near Yazidi country. Dr. Harvey Weiss of Yale directed the excavations there beginning in 1979. The site has been correlated with the ancient city of *Shubat Enlil*. The name is Sumerian, but neither the founding urbanites nor the language need be Sumerian.

NOTES

- 1) See Cyrus H. Gordon, 1962. *Before the Bible: The Common Background of Greek and Hebrew Civilizations*, Chapter VI, "The Minoan Tablets from Crete," pp. 206-271, London; 1966. *Evidence for the Minoan Language*. Ventnor Publishers, Ventnor, New Jersey. Also, Part II of "New Directions in the Study of ancient Middle Eastern Cultures," in: Masao Mori (ed.), *Near Eastern Studies Dedicated to H. I. H. Prince Takashito Mikasa on the Occasion of His Seventy-Fifth*

Birthday (= *Bulletin of the Middle Eastern Culture Center in Japan*, Vol. V, 1991), pp. 53-65. Otto Harrassowitz, Wiesbaden. (Hal wishes to add that Gordon has also championed transoceanic contacts between Mediterranean peoples and the New World. Since neither archeologists nor linguists are willing to accept such things, they are apt to "down grade" anyone who proposes them. *That* may be the reason why a bold yet very careful Semiticist — and one in good standing among Semiticists — cannot get his ventures accepted.) We would like to note that Gordon's views were uncategorically rejected by Yves Duhoux in his book *Les étiocrétois* (1982).

2) A more available discussion of the remnants of pre-Indo-European languages in southern Europe and the islands of the Mediterranean would be useful to have. But in one critical area, we do have an expert opinion. In their joint book on Hurrartean Diakonoff and Starostin write (p.18) that:

"Whatever the initial phoneme, the correspondence is reliable. Note that also Greek πύργος 'tower' is, like many other Greek substratum words, borrowed from Caucasian." Their reference is to what we would transliterate as Greek /púrg-os/ which they relate to proto-Hurrartean */borg-ana/ 'shed, stable'. It was also borrowed into Armenian as /burgn/. For the semantics they refer to the Caucasian custom of keeping "domestic animals on the ground floor of their dwelling towers." For another example, on page 37 they say: "Note Greek σέλας 'light' and σελήνη 'moon' (very probably substratum loan-words)." We transliterate the first as /sel-as/ and the second as perhaps /sel-ini/; they relate it to proto-Hurrartean */sel/ 'moon, moon-goddess' or its counterparts in East Caucasian as 'light'. Carl Darling Buck does not derive either Greek form from proto-Indo-European.

Besides proposing many Caucasian loan-words in Sumerian, mostly via Gutian (Qutian), they cleared up some old questions such as what the ancient Albanian of the Caucasus was. They say: "the extinct Alban (Aghwan) (6) language(s) probably belonged to the same group." The group is the Udi group of the Lezghian branch of Eastern Caucasian. Fn. 6 refers to two articles on Albanian which we list here:

G. Dumézil, 1940-1941. "Une chrétienté disparue: les Albaniens du Caucase", *Mélanges Asiatiques*, vol. 232, fasc.1, Paris, pp. 125-132.

A. G. Šanidze, 1960. "Jazyk i pis'mo kavkazskikh albancev", *Vestnik otdeleniya obshchestvennykh nauk AN Gruz. SSR*, t.1, Tbilisi, pp. 175-186.

Albania was once an important name in the Caucasus, often designating a province of the Sasanian Empire and a district of the Byzantine, showing that after Hurrartean times large political entities still could be associated with minor East Caucasian linguistic entities.

One must note that the Caucasus generally confuses laymen and even encyclopedia writers. Iberia, Albania and Georgia exist both in the Caucasus and elsewhere. Iberia is an ancient name for (Kartvelian) Georgia.

3) See James M. Anderson, 1988. *Ancient Languages of the Hispanic Peninsula*. University Press of America, Lanham, New York & London.

4) See Antonio Tovar, Manfred Faust, Franz Fischer, and Michael Koch, eds., 1979. *Actas del II coloquio sobre lenguas y culturas preromanas de la península ibérica*. The colloquium was held at Tübingen, from the 17th to the 19th of June, 1976. It contains 24 articles by leading Iberialogists.

5) Actually, since Iberian seems to have extended from near Montpellier in southern France to virtual Granada in southeastern Spain, it could be called Eastern Basque. Euskera would then be Western Basque. If Tertesean turns out to be related to them, it could change this nomenclature.

For a valuable new compendium on Celtic, including historically oriented chapters by James Fife and Karl Horst Schmidt, see *The Celtic Languages*, 1993, edited by Martin J. Ball, with James Fife. Routledge: London and New York. Since these authors group Celtic into Insular and Continental, it appears that the "P" vs. "Q" distinction is intended to segregate the Insular Celtic languages. Continental is then not necessarily more closely related to the "P"-Celts. Fife most explicitly refuses to relate Pictish to any particular Celtic subgroup, even though others have mentioned a Pictish affiliation with the Belgae, a continental group.

6) See A. E. Mourant, 1954. *The Distribution of the Human Blood Groups*. (1st edition). Oxford University Press. There is more "N" in West Africa and the western Sahara than there is in central Africa and much less "M" than there is in eastern Africa. The Rhesus haplotype "r" is more common among west Africans and most Bantu than it is among Khoisan-speakers and most Nilotes. On a global scale most of "r" in the world adheres to "Caucasoid" peoples, including most Afrasian peoples and Dravidians. These form a heurism which generally but vaguely support Hans Mukarovsky's general thesis of genetic links in western Africa.

Europe as a whole tends to be a balanced equity between M and N but with slightly more N in the west and slightly more M in the east. The M increases to the southeast and gets fairly high in Sardinia and parts of Italy.

7) See Dimitri Meeks, 1973. "Liste des mots meroïtiques ayant une signification connue ou supposé." *Meroitic Newsletter* July 1973, pp. 3-20.

8) See M. Lionel Bender, 1981. "New Light on the Meroitic Problem." *Meroitic Newsletter* September 1981, pp. 19-24. Some morphemes of Meroitic, cited by Bender as published in Meeks or elsewhere by Bruce Trigger, are given below. The usual precaution that some of these are not so well established still applies. Most are cited with supposed Nilo-Saharan cognates, which are omitted here.

<i>aba</i>	father	<i>abr</i>	man, person
<i>ab</i>	mouth	<i>iy</i>	hand
<i>ad</i>	land	<i>at</i>	food
<i>ende</i>	mother	<i>terikelewi</i>	begotten of
<i>-b</i>	plural	<i>bg, bk</i>	verbal dative
<i>de-, t-</i>	causative	<i>-k</i>	copulative particle
<i>-k</i>	plural	<i>-kelw</i>	also, equally
<i>-li, lo,</i>		<i>-s</i>	genitive
<i>lw, lowi</i>	article	<i>-t</i>	genitive
<i>wi</i>	total, all, big, many	<i>sdew</i>	abundant, all, big, many

<i>mge</i> ,		<i>lx, lg</i>	big
<i>mxe</i>	abundant, all,	<i>tr</i>	big, many
	big, many	<i>x(e)</i> ,	
<i>gr</i>	eat (?),	<i>g(e)</i>	drink (?), pour (?)
	procure (?)	<i>zvqo</i>	foot, footprint
<i>v</i>	give	<i>mle, mlo</i>	good
<i>wid(e)</i>	old; be old;	<i>(ye-)kedi</i>	kill
	infant	<i>glbi</i>	moon, month,
<i>wi</i>	say		season
<i>zgi, zge</i>	small, young	<i>wyeki</i>	star
<i>qbj</i>	Sirius, star	<i>qori</i>	sovereign
<i>ari</i>	sky	<i>mz</i>	sun-god
<i>yere</i>	sun (?), god (?)	<i>pyk</i>	this, this one
<i>tbo</i>	two	<i>mde</i>	maternal uncle;
<i>au, yeu</i>	water		be a parent
<i>sm</i>	wife; be a wife	<i>kdi</i>	woman, female person

Of the first 8 morphemes on the list, some 5 can easily be shown to be Afroasiatic and more convincingly than their Nilo-Saharan counterparts. An Indo-Europeanist colleague noted 5 or 6 strong resemblances to Indo-European, especially Anatolian, including two grammemes. It is interesting that his proposals are mostly the same as those noted by Bender himself, using Indo-European as a kind of control group.

See Bruce Trigger, 1964. "Meroitic and Eastern Sudanic: A Linguistic Relationship?" *Kush* 12:188-194.

9) Modern Egyptians seem to distinguish modern Nile Nubians from themselves. One Egyptian anthropologist (personal communication, Nirvana Khadr 1973) described the Nubians among whom she did field work as a "brown" people and estimated that the Ababda Beja in the eastern hills of southeastern Egypt resembled the Nubians the most. The suggestion is that the ancient Meroeans looked a lot like the modern Nile Nubians, rather than like the Egyptians or like the dark brown or ebony peoples farther south in the Sudan.

Serogenetically, the impressions above tend to be confirmed. Since biogeneticists are often very careless about ethnic and linguistic labels, whole groups of very distinctive peoples get lumped together, e.g., as "Northern Sudanese" in this case. Since Arabs and Beja are usually separate categories, most of the "Northern Sudanese" category is probably Nile Nubian.

10) See F. Højlund, 1993. "The Ethnic Composition of the Population of Dilmun". In *Proceedings of the Seminar for Arabian Studies* held at Manchester, 21st-23rd July, 1992, vol. 23, 1993, pp. 1-8. Published by the Seminar for Arabian Studies, (presumably) the University of Manchester (England). His paper was explicitly addressed to theories of D. T. Potts who had two years before concluded that the Arabian Gulf populations were mixed and that this was due to the intensive trade among three areas, viz., Mesopotamia, the Indus Valley, and the Gulf itself. Note that the world has never decided whether to call it the Arabian Gulf or the Persian Gulf. Maybe we could call the Irish Sea the Welsh Sea or the Saxon Sea.

11) Our trip brought us back to Crete finally because, soon after we finished our report, Allan Bomhard drew our attention to a paper written by Leonard R. Palmer (Oxford) twenty-five years ago. Entitled "Linear A and the Anatolian

Languages," it appeared in *Atti e Memorie del 1° Congresso Internazionale de Micenologia*, Rome, 1968, pp. 339-353 (including comments by other scholars at the end.). Allan was concerned that we might overlook an alternative hypothesis. We were too, but neither of us had poked into all the little philological nooks and crannies in order to do an exhaustive coverage of the Minoan problem. Being busy elsewhere, Dan asked Hal to examine Palmer's paper.

Since my duty was to assess the language evidence more than the epigraphy, I concentrated on amassing that. It was rather disappointing. Thirteen pages of erudite pontification with just a few jots of usable language information in it. He did make a few proposals and a few criticisms. Most important was a statement that: "There are too many uncertainties of reading and sign identification. But Laroche's work on the place-names and the ethnics, together with the historical plausibility of according a major role to the Luvians during the early second millennium, may encourage some to persevere further with a working hypothesis to which we may now give further precision: the language (or one language) of the Linear A inscriptions is an Anatolian dialect which links up in particular with the 'East Luvian' represented by the hieroglyphic texts." [The hieroglyphic texts of Luwian/Luvian, an Indo-European language, not of Minoan. — HF]

Earlier, we criticized those who "have first adopted a hypothetical decipherment and then tested it by looking for pointers to the underlying language." Those were "Furumark, Georgiev, Gordon, Pope, Davis and others..." The solution to the problem of reading the inscriptions "can come only from an analysis of the scanty inscriptional evidence in the hope of detecting diagnostic phenomena. This is the most we can hope to do in view of the paucity of the material and its general nature, consisting as it does largely of place names. In any case, our knowledge of Luwian is so defective that using it to pin down Linear A is like carrying water in a sieve." Diagnostic items come from grammatical analysis, not from superficialities of "vocabulary words" which misled those proposing Semitic links. He quotes Pope, who said: "The trouble may be that those who have been working on Semitic lines have conducted their researches on too superficial a level." (Oom, pa, pa — HF.) Oddly enough, Pope, Palmer, and others at this conference were interested in place names (toponyms) and their analysis. When did we distinguish between toponyms and "mere" vocabulary?

Palmer finds some bound morphemes (suffixes) in the Minoan data, attached to names, and builds his Luwian theory on one or two of them. By themselves, they are not very convincing.

It is interesting that Palmer, the distinguished Indo-Europeanist, barely mentions Gordon, the distinguished Semiticist, and vice versa. (Since Gordon has written scores of articles, I am not positive on this point.)

That Luwian near Adana (Turkey) is a first-rate candidate for Minoan-hood is not disputed. But I'll still bet on Semitic!

SIX GREATER AUSTRALIAN MODIFIED SWADESH LISTS

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We present below the 100-item Swadesh List, as modified by Ken Hale and Geoff O'Grady in 1961, in four mainland Australian and two offshore island languages. These are the Pama-Nyungan languages Nyangumarta (see map in the last issue of *Mother Tongue*) in the northwest of the continent and the extinct Woiwurrung of the southeast; the non-Pama-Nyungan Tiwi of the extreme northern islands Bathurst and Melville, Maranunggu on the mainland to the southwest of Tiwi, and Nunggubuyu along the western shore of the Gulf of Carpentaria; and the extinct, unnamed language of southeastern Tasmania.

Why tamper with the Swadesh List anyway — just for Australia? There are two principal reasons why Hale and O'Grady perceived a need to do so.

First, they wished to avoid situations where two, or even three, items on the List formed part of the referent range of a single lexical item in the target language. Thus 'seed' and 'eye' both come within the purview of Nyangumarta *pani*, and 'mountain' and 'stone' both translate as *warnku* in the same language. Hence, thirteen items required to be deleted from the List:

<i>Deleted</i>	<i>Retained</i>
all, full	many
bark (n.)	skin
cloud, rain	water
drink	eat
feather	hair (of head)
man	person (aboriginal)
mountain	stone
night	black
round, seed	eye
sleep	lie (recline)

Secondly, they wished to exclude, as far as possible, morphologically complex forms, including reduplications of other items from the List. Hence, the following further twelve deletions:

<i>Deleted</i>	<i>Retained or Introduced</i>
bird	(often 'wing-' or 'feather-haver')
come, fly (verb),	
swim	go/walk
die	fall
green	(often a reduplication of 'foliage')
red	(often a reduplication of 'blood')
yellow	(often a reduplication of 'yellow ocher')
white	(sometimes a reduplication of 'ash')
kill	hit (with hand), POTENTIALLY
	killing

know
new

ear, hear
now, today

For a variety of other reasons, Hale and O'Grady also excluded the following seventeen items from the List: 'claw' (in favor of 'fingernail'), 'cold,' 'dry,' 'fish' (not universal), 'good,' 'horn' (not found on kangaroos, etc.), 'hot,' 'louse,' 'neck' (in favor of 'nape'), 'not,' 'path,' 'root,' 'sand' (which kind?), 'say' (in favor of 'speak'), 'that' (proximal?, mid-distal?, distal?), 'walk' (in favor of 'go'), and 'we' (dual?, plural?, inclusive?, exclusive?).

In choosing replacements, the authors of the modified List attached value to meanings which would have *stable monomorphic* counterparts in the target languages. Thus, 'armpit' turns out to be a basic vocabulary item in Australia (and, probably, in Tasmania also) — in stark contrast to the English form or to German *Achselhöhle* or Russian *pod-myš-ka*.

Table 1

The Hale-O'Grady Australian Adaptation of the Swadesh List, August 1961

<i>Nyangumarta</i>	<i>Woiwurrung</i>	<i>Cogn.</i>	<i>Judgment</i>	<i>Gloss</i>
kalguny	WON-GU-RUK	-		armpit
jurnpa	munip	-		ash
ngarlu	puth ~ puj,	-		belly
	piling	-		
wirtu	wurr-wurr,			
	pulatu,			
	wurrthapu	-		big
paji-rni	puntha-	-		bite, to
punyan,	wurrturtin			
kalurru		-		black
pjiirri	kurrk,			
	kurr(u)mul	-		blood
kamari, kunyja	nyilang	-		bone
ngama	pirrm-pirrm	-		breasts
kampi-nyi Vint	nanga(m)pa,			
	TONIMBUCK,			
	CARN.NINE,			
	werrka-ni-wan	-		burn, to
kula	muluku,			
	muluk-muluk	-		by-and-by
minpi	pirring 'breast'	-		chest
karnti-nyi	wirra-, warna-	-		climb, to
ngangkurlu-ji-ni	martu-, marru-	-		cry, to
wirrka-rna	pinta-	-		cut, to
yukurru	yirrangan,			
	wirringan,			
	wirring wilam	-		dog
kaniny	mayi, kuntui	-		down, under
kurlka	wirring, wirn	-		ear
jungka	piik, narrap	-		earth, ground
kakarra	kalen-parriam	-		east
nga-rninyi	thanga-	-		eat, to
(PRES)				

jimpu	tirrantirr (also with -l, -n)	-	egg	raminy	(nyilingi) tarnin 'bone'	-	rib
yurlku	kurrun, paluth	-	elbow	piminy, puka	puang, warrupak, puterrinin...	+0.17	rotten — as meat
purta	ku(r)na(ng)	-	excrement	yirri-rni	nganga ~ nanga...	-	see
pani 'eye, seed'	mirring	-	eye	murlku	murt(a)	-	short
pungki-nyi	pulta-, patherrempi	-	fall, to	kaja-rna	ngala(m)pa-, muruntaka	-	
kaja	WARRIT, jiyu	-	far		'stay, live at'	-	sit, dwell, to
milpiny	thirrip	-	finger nail (subst. for 'claw')	karnu	thaap, morrok	-	skin
wika	wiiny	-	fire	parlparr	wurru-wurru, TOTE.BARE.RIB-	-	sky
warrayi	karrakarrak, karramparra	+1.0	fly (insect)	wupartu	wayipu, waikurk-	-	small
kuyi	wingkarram	-	flesh, meat	parnti-rni	ngarropa-	-	smell it, to
mayi	thanguth	-	food (non-meat)	tuyi, jungan,			
jina	jinang	+1.0	foot	ngunyjirr,			
pitapita,				yukurn	purt	-	smoke
pirriri	minyini	-	forehead	jurru	kurnmil, kaan,	-	
ma-ninyi (PRES)	muka-	-	get (pick it up)		kulunung	-	snake
yi-nginyi-...-a	wunga,			kurili	kurrin, mirin,	-	
(PRES)	yuma(rra)la	+1.0	give		wapurn	- (sic)	south
ya-ninyi (PRES)	yana-, tuwi-, jithu,			muwarr-pi-ni	thumpa-, turnmin-	-	speak, to
	KORRRNANG			muntu	wulip,		
	-NGI	+0.25	go, walk	janga	warra-warra	-	spear
pajarli, wari	ma(rr)mpul	-	grease, fat	wararr-karri-nyi	JUG-AN-DAK	-	spit, saliva
mampu	yarra	-	hair (of head)	pani-kiti	tharri(ji)-	-	standing, to be
parirr	marnang	-	hand	(pani 'eye')	turt, turt-pairram	-	star
tarlakarra	palit, palert	-	hard (as ground)	warnku	la(a)ng, muyijirr	-	stone (also 'mountain' in some languages)
juju	kawang	-	head				
pinakarri-nyi	ngarnka-, ngaku- ≈		hear	karrpu	ngawany	-	sun
turlpu	turru(ng)	-	heart	warnti	MOIBO,		
wirla-rna	jilpa-	-	hit, kill		MOOEBORREN-	-	tail
ngaju	wan, marramb-ik	-	I	kalparti	jarrang, jirrong	-	thigh
murtingi	parring	-	knee	nyungu	wu, maayu,		
walyaka	jerrang, marran	-	leaf		NOTTO... 'here'	-	this
yaka-rna-...-a	—	0	leave it, to	rungu,			
karta-karri-nyi	yimu-, ngaikul	-	lie, sleep, to	runganrungan	thalapi-kurn	-	throat
wanpa	pulij	-	liver	jarlin	jalang	+1.0	tongue
makanu	yuripot, nyirrirrim,			rirra ~ yirra	liang	+1.0	tooth
	tung-tung	-	long	wurru	tarrang, kalk	-	tree
marlu	wurrthun,			kujarra	pinjirru	-	two
	wurrtiyalyal	-	many	kanka	kaputh,		
tartarta	mirnian, yampuk	-	moon		ngirr-ngirrwan,		
jawa	wurru(ng), kantana	-	mouth	nguntu	kupi	-	up, above
yini	narrin	-	name	ngapa, warnayiti	palk(a)	-	urine
taki	NANG-ING-A-TA 'our neck'	-	nape of neck		paany 'water,'		
yalinyji	parraji, winmali	-	north	kara	panyimin 'rain'	-	water, rain
milya	kaang	-	nose	ngani	mumil(am)	-	west
kuwarri	yalingpu, NETBO, MANGEE	-	now, today	wanyjarni	winha	-	what
	wikapil	-	old man	nganurtu, ngartu	winytha	+1.0	where
ranyji	kanpu, kup(-tun)	-	one	wangal	winharrup	-	who
waraja	kuliny	-	person (aboriginal)	ngalyun	murnmut	-	wind
marrngu				nyuntu	pajurr, pakurk	-	woman
					warr,		
					marramb-inherr	-	you SG

6%

Table 2

Tiwi - Adapted List

1. kulingərrima	armpit	ground
2. pumutinga	ashes	head
3. wurarra, pitapita, awərri-	belly	hear, to
4. arikulani 'big, old, senior, important, big man, elder,' yingkurti 'big, old, senior, adult,' arrəwələpuni 'big man (i.e., important)'	big	heart
5. -wirri 'bite, sting'	bite, to	hit, kill (with hand)
6. tuniwini 'black, dark (e.g., dark blue, dark green), black man'	black	46. ngia 'I, me, my, mine,' ngə- ~ ngu- ~ ngi- ~ ngəm- (subject prefix) I
7. pulini ~ yimpulini (archaic)	blood	47. yimpula, pula- knee
8. pwatha 'bone, shell ...'	bone	48. wiyini, pərrarti leaf
9. pularti 'woman's breast, milk,' pungini-breasts		49. -irampira 'leave (something) behind' leave it, to
10. wurrika 'hot, burning,' -wa 'burn' (Vintr) burn, to		50. -mathərripi 'lie down' lie, sleep, to
11. —	by-and-by	51. pwathukura liver
12. maripə-, tawunalua (archaic)	chest	52. yərrukuni, murruputi 'long, tall' long
13. -akəlinga, -awilari	climb, to	53. taikuwani 'many, a lot of,' punhthiwi (archaic), arralupuwi (archaic), yingarti 'plenty of, a lot, much, full' many
14. pilingkiti (verbal noun) 'cry, weep, wail,' -ukuntirri cry, to		54. thaparra, thaparrulini moon
15. -awurrini 'break in two (tr.), cut,' -makanha 'break, cut, split, tear' cut, to		55. yərrəpu(n)tara, yərrəpu(n)tikawa, əpə- mouth
16. pəlangəmwani, kətarriŋani, wangkini, pamulampunhini dog		56. yintangā name
17. —	down, under	57. —
18. mika(nh)thanga, pərrakuninga, pungitə-ear		58. —
19. yakuluwuni, yarti	earth, ground	59. yərrəngəntamura, əngintami- nose
20. —	east	60. waiya 'already, now,' ningani 'today, now, nowadays' now, today
21. -apa, -mulatha, mwanhthə(ngə)-, ngamungami (verbal noun) 'eat, drink' eat, to		61. yurrula, purrumarti (archaic), parlini 'old, old man, ancestor, tradition' old man
22. karaka 'turtle's egg,' kəluwuka 'turtle's egg,' əngənəngi- 'turtle's egg' egg		62. nhatinga, yati 'one, alone, both' one
23. yəəmpunga	elbow	63. tini 'human being (male), man' person (aboriginal)
24. kinhirri, puntə-	excrement	64. yali(m)parna rib
25. pithara, mingkika, kəli-, tupurra (archaic) eye		65. piriŋpani 'bad, rotten, decayed, foul' rotten — as meat
26. -akupuranhthi 'fall down,' -awuliŋi, pwapuua 'fall down' fall, to		66. -əmani, -uputhi see, to
27. karrampi 'a long way, a long time, far, distant' ... far		67. karuwuni 'short, short man' short
28. kərumunaya, munaya	finger nail	68. -'mu 'sit, sit down, stay, live,' -məringarra 'sit, stay, live' sit, dwell, to
29. yikwani 'fire, firewood,' ki-	fire	69. mipurra ~ mupurra, tawila, puliminta (archaic) skin
30. upwani	fly (insect)	70. wampaka, yungungkwa sky
31. puningkapā, yinhthulota (archaic)	flesh, meat	71. kirithini ~ kiithini 'small, little, young, little boy' small
32. yingkiti 'food,' muwunikini 'food' food (non-meat)		72. -wantia smell it, to
33. milampwara, kəntanga, mili-, milakintanga (archaic) foot		73. kumurripini smoke
34. wuratinga, wurati-	forehead	74. taringa 'general term for poisonous snakes of unknown species,' aruwunga 'poisonous snake (all unidentified species)' snake
35. -unga 'grab, get, catch, take' get (pick it up), to		75. — south
36. -akərai, -ilua, ani (verbal noun)	give, to	76. -angəraya speak, to
37. -u'ri, -mi 'do, go, say' go, walk, to		77. mungarla(ng)ka, walangka, numwariyaka spear, a
38. tangarra, yimpwarla	grease, fat	78. thuwari spit, saliva
39. murrula	hair (of head)	79. -inti 'stand, stand up' standing, to be
40. yikara, wamuta, kəri-	hand	80. thapalinga star
41. -kətarumi 'be strong or hard,' -urumi 'be strong or hard,' ma(ng)karrana 'hard, vigorously' hard — as of		81. waranga stone, mountain
		82. yiminga 'sun, breath, life, totem,' pu'kwi 'sun, totem' sun
		83. tuwara, aripia tail
		84. yingkala, kəripayua thigh
		85. ngəna(ng)ki 'this, this one' this
		86. məraka throat

87. yimitarla ~ mitarla, apathi-	tongue	35. pej 'to get, have, hold, carry, marry'	get (pick it up), to
88. yingkana, yiliringuwana ~ liringuwana 'tooth, barb'	tooth	36. wut 'to give; to put down on the ground,' wurut 'to give (several objects)'	give, to
89. purinhtherringa 'tree (general term), log,' taka, ə(ngə)- 'tree, log, dugout canoe'	tree	37. paraj 'to chase away; to follow; to creep up on; to go,' wat 'to go, to walk'	go, walk, to
90. yirrara	two	38. reri	grease, fat
91. kəri'u	up, above	39. piyamerr, merr	hair (of head)
92. pwathini, mulamini (archaic)	urine	40. niyungku 'hand, finger'	hand
93. mangala, kukuni 'fresh water,' mangelumpi 'fresh water' (archaic), mangu- 'fresh water,' winga 'sea, salt water,' murrupaka 'sea, salt water'	water, rain	41. elmetter 'hard, tough'	hard — as of ground
94. —	west	42. piya	head
95. kamini 'what, which?,' aungwana 'how?, what?' what		43. —	hear, to
96. maka	where	44. mirijun	heart
97. kuwani	who	45. kur 'to shoot, hit,' kurkur 'to hit, strike'	hit, kill (with hand)
98. wuninhthaka 'wind, breeze'	wind	46. ngany	I
99. yimparlinha 'woman, female,' tinga 'human being (female), woman,' ngarikətəmənina	woman	47. pingkarra	knee
100. nginhtha 'you (sg.), your, yours,' nhə- ~ nhəm- ~ nhem- ~ nh- (subject prefix) 'you (sg. or pl.)'	you SG, thou	48. kalkal	leaf
		49. kal 'to leave, take out of the water'	leave it, to
		50. —	lie, sleep, to
		51. perele	liver
		52. malikita	long
		53. tap 'many; a lot'	many
		54. alamuny 'moon, month'	moon
		55. ngal 'mouth, door'	mouth
		56. winka	name
		57. manta teti	nape of neck
		58. —	north
		59. yin	nose
		60. nini 'now, at the present time,' pentere 'now; for a long time,' kujala 'today'	now, today
		61. muntak 'old; certainly, in fact, really; some time ago'	old man
		62. ngantawany	one
		63. —	person (aboriginal)
		64. lorrmin 'ribs'	rib
		65. perkuriny	rotten — as meat
		66. —	see, to
		67. wontopor	short
		68. tat 'to sit down, rest, to stay'	sit, dwell, to
		69. teripiriny	skin
		70. —	sky
		71. kiruwali 'small, little'	small
		72. nyunyuk	smell it, to
		73. jumu	smoke
		74. mala	snake
		75. —	south
		76. marany 'to speak (a language)'	speak, to
		77. jinta	spear, a
		78. tiralk	spit, saliva
		79. yarung 'to stand up straight; straight'	standing, to be
		80. moro	star
		81. karawala 'stone; money'	stone, mountain
		82. kiyik	sun
		83. yiri	tail
		84. tarr	thigh
		85. keni, kenki	this

Table 3

Maranunggu - Adapted List

1. wenter	armpit	62. ngantawany	one
2. yiminy tarra 'hot ashes,' tarrajujurr 'cold ashes'	ashes	63. —	person (aboriginal)
3. lurr 'belly, stomach'	belly	64. lorrmin 'ribs'	rib
4. puwal 'big, large'	big	65. perkuriny	rotten — as meat
5. karrkarr	bite, to	66. —	see, to
6. jipme	black	67. wontopor	short
7. purwur	blood	68. tat 'to sit down, rest, to stay'	sit, dwell, to
8. mu	bone	69. teripiriny	skin
9. yingi 'breasts, milk'	breasts	70. —	sky
10. yuk	burn, to	71. kiruwali 'small, little'	small
11. —	by-and-by	72. nyunyuk	smell it, to
12. maritemperr	chest	73. jumu	smoke
13. kalkal	climb, to	74. mala	snake
14. wirija	cry, to	75. —	south
15. kat 'to cut; to chop (wood),' tur	cut, to	76. marany 'to speak (a language)'	speak, to
16. mi	dog	77. jinta	spear, a
17. penpe	down, under	78. tiralk	spit, saliva
18. jengi	ear	79. yarung 'to stand up straight; straight'	standing, to be
19. pitlam	earth, ground	80. moro	star
20. —	east	81. karawala 'stone; money'	stone, mountain
21. jakal, jam 'to eat (of an animal)'	eat, to	82. kiyik	sun
22. muru 'egg; testicles'	egg	83. yiri	tail
23. pontor	elbow	84. tarr	thigh
24. wun	excrement	85. keni, kenki	this
25. miri 'face, eyes; seed; to look for; to look at'	eye		
26. —	fall, to		
27. ngaytpirr 'far away, distant'	far		
28. —	finger nail		
29. yiminy 'wood, fire'	fire		
30. kalanguk	fly (insect)		
31. awa	flesh, meat		
32. miya	food (non-meat)		
33. kumpu	foot		
34. rimi	forehead		

86. manta 'neck, throat'	throat	far
87. ngaltiritiri	tongue	finger nail
88. tirr	tooth	fire
89. tawar	tree	fly (insect)
90. miyitiny	two	flesh, meat
91. —	up, above	meat
92. ajawa 'urine; to urinate'	urine	eggs, tree gum, etc.)
93. wuta	water, rain	food (esp. flour, sugar, porridge, vegetables)
94. —	west	food (non-meat)
95. emi, enji	what	foot
96. antama	where	forehead
97. apa	who	to pick up, get, take, janggaw! 'to pick up, take (in one's hand), jaw! 'to pick up, grab'
98. warmala	wind	get (pick it up), to
99. peku	woman	to give to, to hand (something) to
100. nina (subject), -nimpe (object)	you SG, thou	give, to

Table 4

Nunggubuyu - Adapted List

1. anyjabal, -(w ₁)anyjabal-, -anybal-, -anyjal-	armpit	
2. lhagabuṇulg 'dust; ashes'	ashes	
3. gulmung, dhulmung	belly	
4. runggal, -w ₂ ugag 'big, vast'	big	
5. gaḍ!, =w ₂ a- 'to bite; to hold (object) in teeth'	bite, to	
6. du-duma-j 'black, dark'	black	
7. w ₂ ulang, nguluji	blood	
8. ngagara	bone	
9. miimi 'milk; female breast'	breasts	
10. =nagi- 'to burn, be on fire; be extremely hot or bright'	burn, to	
11. —	by-and-by	
12. wunyan 'upper chest (of body), w ₁ urij 'chest (of body), esp. mid and lower part'	chest	
13. =adada- 'to go up slope, =w ₂ ida- 'to go up (esp. in vertical direction, not up slope), =w ₂ albalma- 'to climb up (tree, etc.)'	climb, to	
14. =rugu- 'to weep, cry, =lharbijija- '(tears) to fall down from (person)'	cry, to	
15. =balhu-, =aayu 'to cut up (meat, etc.), to cut into, to butcher'	cut, to	
16. landhurg	dog	
17. lhiribala 'underneath, inside'	down, under	
18. w ₂ arang	ear	
19. aban 'ground, dirt, earth, -a(G)- 'ground, dirt, earth, -(w ₁)aban 'ground, dirt, earth'	earth, ground	
20. ramali 'east, in the east'	east	
21. =ngu- 'to eat; to swallow, gulp down, ngam!, =w ₂ a-	eat, to	
22. gagaḷang, maabu, gaḷa-maabu, gaḷang-	egg	
23. mulung	elbow	
24. nguriya ~ ngurya, -gi-	excrement	
25. bagaḷang, muṇbarg, -bag-	eye	
26. =rabi- 'to fall down'	fall, to	
27. malanga-nyanay ~ malanga-nyanaj 'distant, far away'		
28. yara 'fingernail, toenail'		far
29. ngura 'fire, firewood, -yiga-		finger nail
30. aamuny 'the common bush fly'		fire
31. lhanggu, yalaj 'meat as a change of diet'		fly (insect)
32. marya 'soft food (general term including fruits, vegetables, eggs, tree gum, etc.), -w ₂ aj- 'food (esp. flour, sugar, porridge, vegetables)'		flesh, meat
33. muun, -lhan-		eggs, tree gum, etc.)
34. ranggal		food (non-meat)
35. =ma- 'to pick up, get, take, janggaw! 'to pick up, take (in one's hand), jaw! 'to pick up, grab'		foot
36. =yi-, =w ₁ u-, =barguga- 'to give to, to hand (something) to'		forehead
37. =ya(a)- 'to go, =ruma-, juj! ~ juy!		to pick up, take (in one's hand), jaw! 'to pick up, grab'
38. malagaj '(thick) honey, liquid fat or grease or other thick liquids'		get (pick it up), to
39. muung		give, to
40. marang 'hand, finger'		go, walk, to
41. w ₂ aḍawadad 'strong, firm, solid, hard'		grease, fat
42. -ambulug-, ambara, yinang '(top of) head, amburug '(top of) head, ambal '(top, crown of) head, laang '(top of) head'		hair (of head)
43. =yanga- 'to hear, listen to (someone, something); to understand or remember (something); to think about'		hand
44. andhiri		hard — as of
45. =w ₂ u- ~ =u- 'to hit hard; to kill, to shoot'		ground
46. ngaya		head
47. laan		hear, to
48. w ₁ ujiyar-, yir- 'leaves, foliage'		heart
49. =aaru 'to abandon, to leave behind'		hit, kill
50. =munymulha-, =murgulha-, =w ₂ aḷarlha- 'to lie down on back...'		(with hand)
51. aman, -w ₁ aman-		I
52. jarmayarmaj 'long, tall, lhaalun 'long and straight, rod-like, -ngaar- 'long object, rod-like object'		knee
53. w ₁ arawindi ~ arawindi 'much, many'		leaf
54. labama, ngalindi		leave it, to
55. ramadhan, -lha- ~ -lhag-		lie, sleep, to
56. muwaj, mij-		liver
57. nindhagal		many
58. wunumbi 'in the north'		moon
59. yimurg		mouth
60. aḍaba ~ aba 'just now, just then, jimbaj 'today, now, nowadays; the same day (as something else)'		name
61. yiwanggu ~ yiyanggu		nape of neck
62. anyjaabujij 'one (numeral); single, by oneself'		north
63. w ₂ uruj		nose
64. —		rib

65. dhadulg 'stale, rotten, old,' -lhurung- 'large chunk (of meat); rotten meat,' nunulg 'rotten, stale,' w ₁ uril 'rotten, rotten-smelling or -tasting'	rotten — as meat	4. proi(na), nūmena, pāpela, pali	big
66. =na-	see, to	5. reli-kara	bite, to
67. dhamurug 'short; (for a) short time (adj. or adv.),' -mulumulubug 'short (in height)'	short	6. maupa(k)	black
68. =bura- 'to sit down; to be sitting, to be (in a place); to stay,' =ambargala- '(two or more) to sit down (in group)'	sit, dwell, to	7. kókā	blood
69. magulag 'skin; (tree) bark'	skin	8. téne(na), teni(na), TOODNA	bone
70. -mala- '(clear) sky,' yałamara	sky	9. perūga, peróga(na)	breasts
71. w ₂ irig, w ₂ inyig 'small, tiny; (human forms) child,' yiryiragij 'small (ones)'	small	10. WUGGATAH 'hurt by fire'	burn, to
72. =yara- 'to smell (something); to detect, to sense (something)'	smell it, to	11. GUNNYEM WAUBBERABOO	by-and-by
73. -ban- 'smoke, steam'	smoke	12. TOORINAH 'breast (chest)'	chest
74. maarny 'snake, rainbow'	snake	13. krongana	climb, to
75. wagay 'southward'	south	14. toni, ló-gata, tára; ta'ara(na)?	cry, to
76. =yambi-	speak, to	15. toa-gara, poinga	cut, to
77. larda	spear, a	16. kuayáto, ¹ mūkra, panoine	dog
78. lhagar	spit, saliva	17. lute, lumna	down, under
79. =lha- 'to stand, to be standing'	standing, to be	18. wayi, koigi, tiberatie	ear
80. miyiri 'star (includes planets)'	star	19. māra(na), mánena, wébörē	earth, ground
81. nuga	stone,	20. NIR.TER	east
82. alir	mountain	21. tó-gera	eat, to
83. rabara, ardha 'tail (of dugong, whale, or porpoise) tail	sun	22. patīnā	egg
84. lharbij	thigh	23. wayeni(na)	elbow
85. —	this	24. tiá(na), tī(na)	excrement
86. ramaj, yambiya	throat	25. nüberē(na)	eye
87. —	tongue	26. TONKA 'tumble'	fall, to
88. raa	tooth	27. WEBBERY	far
89. rangag 'tree, shrub (any woody plant); wood; stick'	tree	28. toñē 'fingernail,' kōlógana 'claw'	fingernail
90. w ₂ ula- ~ ula-	two	29. toi(na) 'light, fire'; (ng)una 'fire' ²	fire
91. arwar 'above, on top; uphill, upriver, inland' up, above		30. wilē, wīlīna	fly (insect)
92. raaj 'urine, bile'	urine	31. kragana	flesh, meat
93. guugu '(fresh) water,' lhagayag 'salt water; sea' water,	rain	32. —	food (non-meat)
94. argali 'in the west'	west	33. lóga(na)	foot
95. yangi 'what?, which one?'	what	34. rou'ena, roiróna	forehead
96. a-ji-ga	where	35. tia-kara	get (pick it up), to
97. yangi-nyung, yangi-nyum-baa (dual), yanga-yangi (plural)	who	36. tiéna	give, to
98. -lhanguny	wind	37. ta-we, tá-kerā, tātabūrena	go, walk, to
99. mañi-nyung 'female; woman'	woman	38. PAR.NIN.NER.RE	grease, fat
100. nagang	you SG, thou	39. pōle	hair (of head)
		40. rī(na), nána	hand
		41. WEERULLÉ 'firm, not rotten'	hard — as of
		42. poyete	ground
		43. wayī 'ear'	head
		44. NO.MY, TEGGANA	hear, to
		45. lō-gana, mene-gana	heart
		46. mī(na), ma(nā)	hit, kill (with
		47. ranga	hand)
		48. peroyé	I
		49. wana- ? ³	knee
			leaf
			leave it, to

Table 5

Southeastern Tasmanian (Schmidt's reconstruction)

1. kawálā	armpit
2. TOIBERRY	ashes
3. lomáti(na)	belly

57. lorēna 'neck'	nape of neck
58. NAR.PINE.NER.TAR	north
59. mud'e(na), mögenā	nose
60. —	now, today
61. wanxôte, paiyena 'old'	old man
62. mára(-wā), pāmere	one
63. weiba, pálawā, nagata, nōgana	person (aboriginal)
64. tené	rib
65. —	rotten — as meat
66. nubratoné	see, to
67. LOUGH.WE	short
68. krá-kana	sit, dwell, to
69. lowari(na), liwóri(na)	skin
70. wára(na)	sky
71. pawī, pūtē, rie(le), ri(na)	small
72. poina 'smell'	smell it, to
73. būrana, pūdā	smoke
74. loina 'black snake'; paweraḵ 'diamond snake'	snake
75. LY.DID.DER	south
76. ongi, poyata	speak, to
77. pena, prīna	spear, a
78. nūkera 'to spit'	spit, saliva
79. pegera 'stand up'	standing, to be
80. daledina, romtóna	star
81. po(a)ta(na), lóna 'stone'; layetē(na) 'mountain'	stone, mountain
82. palla-nubranā, panubere	sun
83. PUGHNAH (?)	tail
84. tōxrā, TEIGNA	thigh
85. LONOI	this
86. WUN.NER.ER	throat
87. ména	tongue
88. payī(na)	tooth
89. luparī, wī(na)	tree
90. pūa(li), pwa(li)	two
91. CROUGANA 'aloft'	up, above
92. mönga(na)	urine
93. lia(na) 'water'; póra, būra 'rain'	water, rain
94. TONE.EN.ER	west
95. aḥā	what
96. WABBARA	where
97. —	who
98. rála(na), ragala(na), ra'ála(na)	wind
99. tībra(na)	woman
100. nīna	you SG, thou

(94% of the needed 100 items appear in the above)

- 1) The dingo/dog was not indigenous to Tasmania.
- 2) Western Tasmanian (TAS-W) (w)una(leā) 'fire.'
- 3) TAS-SE WANNABAYOOERACK 'forget.' Note
TAS-W WANNABEATONGH 'abstain' — semantically
suggestive of 'leave it.'

Table 6

Putative Cognates

bite, to	NYA — NUN (=w ₂ a-) TAS-SE — MAR
blood	WOI — TAS-SE NUN (w ₂ ulang) — TIW
burn, to	WOI (nanga(m)pa) — NUN (=nagi-)
chest	TIW (maripā-) — MAR
earth, ground	TAS-SE (wébörē) — NUN (-(w ₁)aban)
eat, to	NYA — NUN (ngam!) — TIW (ngamungami) WOI — MAR (jakal)
excrement	NYA — TIW (puntā-)
eye	WOI — MAR
fire	NUN (-yiga-) — TIW (yikwani)
fly (insect)	NYA — WOI
food (non-meat)	NYA — MAR
foot	NYA — WOI
get (pick it up, to)	NYA — NUN (=ma-)
give, to	NUN (=w ₁ u-) — MAR NYA — WOI (yuma(rra)la) — NUN (=yi-)
go, walk, to	NYA — WOI (yana-) — NUN (=ya(a)-)
hair (of head)	TIW — MAR (merr)
hard — as ground	NUN — TIW (-kəturumi)
head	TAS-SE — MAR
I	NYA — NUN — TIW — MAR
leaf	TAS-SE — TIW (pərrarti)
moon	NUN (labama) — TIW
nose	NYA — TAS-SE
rotten — as meat	NYA (puka) — WOI (puang)
sit, dwell, to	WOI (muruntaka) — TIW (-məringarra)
small	NUN (w ₂ irig) — TIW — MAR
speak, to	TAS-SE (ongi) — TIW
spit, saliva	WOI — TIW
stone, mountain	TIW — MAR
tongue	NYA — WOI
tooth	NYA — WOI — TIW (yiliringuwana) — MAR
tree	TIW (taka) — MAR
two	TAS-SE — NUN
water, rain	NUN (guugu) — TIW (kukuni)
where	NYA — WOI
woman	WOI (pakurrk) — MAR
you SG, thou	NYA — TAS-SE — MAR

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IS SAAMIC *kuovča* 'BEAR' A DENE-CAUCASIAN LOAN WORD?

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1. Our forefathers (in Europe) were so well acquainted with the wolf, badger, fox, and bear that they became main characters in many animal fables. Today, of the mammals which used to be distributed over large areas of Europe during the Ice Ages, reindeer, musk-ox, and Saiga antelope are now found only in the far north or Inner Asia, whereas wisent, brown bear, wolf, and ibex are rare, almost exterminated in Central Europe. The acquaintance vs. non-acquaintance with these mammals in prehistoric time may also give us some information about these societies and their possible origin.

2. When investigating the relationships of Gilyak, the language isolate on the island of Sakhalin and at the mouth of the Amur River, Karl Bouda (1960) stressed the large number of matchings with Uralic (especially Samoyed and Vogul). As far as Saamic was concerned, the arctic terms involved seemed remarkable: In addition to three words for 'seal,' there was Gilyak *'khuz-r* 'Vielfraß' A[mur dialect] mit nominalem Suffix aus **kus*," matching Saamic *kuovča* 'bear' (Bouda 1960:379). Bouda was not able to give an Uralic etymology for the Saamic word — maybe because it is a loan word...

3. Bengtson (1991:103) lists two Dene-Caucasian reconstructions for 'bear': (1) ***X^warC-* and (2) ***c^hnk^w-* (?). In a later addition, he points for (1) to a possible Indo-European connection (IE **H₂kt-o-* ~ **H₂rk-o-* 'bear'; cf. also Finno-Ugric **okte* id.; Bengtson 1991:121). More relevant for Saamic *kuovča* seems to be North Caucasian (Daghestan **XT^wVrčV* 'squirrel, marten': Dargwa *XaIrc* 'squirrel', etc.; Bengtson 1991:103). As in *kuovča*, *-uo-* reflects earlier **a* (cf., e.g., North Saamic *guokte*, Finnish *kaksi* 'two'), the Saamic matching with Dene-Caucasian becomes more transparent. Of Na-Dene and Almosan-Keresiouan matchings, one could point to Haida *XuužI* 'grizzly bear' Tlingit *Xuuc* id. (cf. the above Gilyak form) (Bengtson 1991:103) and Algonquian: Proto-Central Algonquian **maxkwa* 'bear', Salish: Coeur d'Alene *-maχi[?]čən* 'grizzly bear' (Greenberg 1987:166).

4. Maybe this little animal story can add something new to the question of the origin of the Saami, speaking (today) a Finno-Ugric language but being racially distinct from the other Finno-Ugric speaking people (such as the Hungarians).

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NOTE

Archaeological excavations in the Hammerfest region (Norway) have yielded (pre-)Saamic datings of about 8,000 B.C. (Björn Olsen, Tromsø Museum, in Swedish Television TV2, 24 October 1993). It would be interesting to hear the reaction of S. A. Starostin, who in his 1990 Cold Spring Harbor paper, on the basis of lexicostatistics, proposed to date the divergence of Proto-Uralic and Proto-Altaic to about 6,000 B.C.

MAORI *kaipuke* 'SHIP' AND ESKIMOLOGY

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Many (Maori and Pakeha) readers of the *New Zealand Herald*, 21 January 1988, must have been extremely surprised to be told that "Maoris and other Polynesians may have Eskimo ancestors..." What should have been an abstract of my paper to be presented at the Auckland Conference, turned out to be a journalist's "subtraction". "Early Eskimo in Hawai'i (?)" had been the general issue (Schuhmacher 1988), and the term "Maori" had not come up once in that connection. Therefore, being wiser now, I want to stress that the following remarks should not be interpreted along the journalist's line as "evidence" for "Eskimo in New Zealand".

Robert Langdon (1988) believes, among other things, that Maori accounts of the celebrated Tainui canoe record the story of the Spaniards' arrival in New Zealand. Thus, according to him, the distance between the limestone slabs (76 feet) at Kawhia where the Tainui is supposed to have been "buried" is approximately the length of a 16th century caravel. And, consequently, the Maori word for 'ship', *kaipuke*, should represent a loan from Spanish (that has *buque*).

What could be said then about the etymology of *kaipuke* from a Polynesian point of view? *kai* may be identified with Proto-Polynesian (PPN) **kai* 'tree, wood' — or with PPN **kai* 'food, eat'. The latter leads me to the Eskimo...

Eskimo *qajaq* (with variants) 'kayak' probably is the most famous word of the language. It even occurs in almost identical shape meaning 'little boat' in Turkish, and in other Altaic languages (e.g., Ewenki *kajuk* 'bark canoe'). The

cognate Aleut word is /iqyaʃ/ — and this one, originally probably *iqajaq* ('tool for the catch of fish/food?'), could have been the source of the Eskimo word and the Altaic loans.

kaipuke as 'food/fish catcher'? (Cp. Hawaiian *pu'e* 'attack'). The "wrong" word-order could point to a Maori loan from another language, cp., e.g., Rapanui *te tangata kai* 'the man-eating', *te kumara keri* 'the kumara digging'.

Thus, referring to the introductory remarks, I do not posit any genetic connection between *kaipuke* and *qajaq*. (To posit one between Proto-Austronesian **ikan* 'fish' and Eskimo *qajaq* Aleut *iqajaq* would be another story...)

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THE DENE-CAUCASIAN RECONSTRUCTION FOR 'MOON'

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1. Following in the wake of his countryman W. J. van Eys, C. C. Uhlenbeck (1903:57) explained the Basque word for 'moon', *illargi*, etc., as composed of (*h*)*il* and *argi* — an explanation he later in his "Corrections" (Uhlenbeck 1923:7) questioned again when turning to the old etymology 'light of the dead'.

2. Bengtson (1991:99) lists "Basque *hil* ~ *hila* 'moon, month', *hil-(argi)* 'moon, moonlight'...", and compares it to Burushaski *hal-* 'moon' and Sino-Tibetan forms (even listing other extra Dene-Caucasian matchings by others).

3. Not mentioned by Bengtson is Proto-Polynesian (PPN) **maa-sina* 'moon' (where PPN **sina* 'white/gray-haired' [Walsh and Biggs 1966:100] < Proto-Austronesian **t'inay* 'light' [Dempwolff 1938:154]). Benedict (1990:219) renders the Proto-Austronesian reconstruction as **t'ilay* (< Proto-Austro-Kadai **[ts, tʃ]ilaR* 'light/shine/white/gray (hair)'; Japanese has *sira-* 'white, gray').

4. Whereas Bengtson (op. cit.) has ***h₁ila-* as Dene-Caucasian proto-form, incorporating the Benedict reconstructions, ***[ts, tʃ]ila-*, seems to be more appropriate (cf., e.g., Schuhmacher and Seto [1993] on Austronesian and Dene-Caucasian).^{1, 2}

NOTES

1) I want to leave it to others, maybe within the framework of Proto-World, to comment on Quechua *kil'va* 'moon'; on *Si*, moon-god of the Chimú of ancient Peru; and, last but not least, on *Sin* moon-god of the biblical Ur of the Chaldees.

2) *ts, tʃ* = Bengtson's *c, č*.

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MORE ON MATTERS INVOLVING INDO-EUROPEAN (IE) ARCHEOLOGY

Kenneth Jacobs, writing in *Current Anthropology* 34, Number 3, June 1993, pp. 311-324, presented "Human Postcranial Variation in the Ukrainian Mesolithic-Neolithic." He is at Département d'anthropologie, Université de Montréal, CP 6128/Succ. A, Montréal, Québec, Canada H3C 3J7. There is no abstract.

Jacobs is primarily interested in relating the abundant data on the people of the Ukraine to their contemporaries elsewhere — during the Mesolithic and early Neolithic. Eventually, one realizes that his old Ukrainians are in fact the speakers of Proto-IE, and his goal to account for them, using the impressive cemeteries of the Mesolithic period to establish a baseline population against which to relate the Neolithic folk.

Since many studies had concentrated on cranial materials, his particular focus was on the long bones, to wit, clavicle, humerus, radius, ulna, femur, and tibia. But he also cites dental studies for additional information. After long, detailed finely honed analyses, buttressed by a multiplicity of statistical controls, he concludes three interesting things:

1. Mesolithic Ukrainians were taller (174-176 cm) than their west European counterparts (167-168 cm) or about 3"

taller for adult males on average. The ratios applied during the Neolithic and for females.

2. Between the Mesolithic and the Neolithic, the Ukrainians did not get taller at all. However, they definitely got more robust; bones got bigger and heavier. Males as well as females.
3. Their "robusticity" (awful word!) was not due to some unknown migration of "Cro-Magnons" who might have been hiding out in the northern forests (as some Soviet thinkers supposed). Our Ukrainians got robust from hard agricultural work!

This was not due to pressure from or acculturation to "out-of-Danubia" farmers, however. The Ukraine got agriculture earlier than their Danubian neighbors to the west and southwest. By very careful and recent "accelerator carbon 14 dates on human collagen from bone samples," they found the Mesolithic skeletons from Vasylivka dated to 10,000 B.P. ("uncalibrated radiocarbon years"), while the Neolithic skeletons had dates of 7850 B.P. \pm 200 years (also uncalibrated). That is too old to be derived from the Danubian Neolithic. This is an important finding. Jacobs had quite a bit more to say, leading swiftly to a bold new theory of Indo-European origins. It is one which, oddly enough, could satisfy both the Gimbutas and Renfrew-Cavalli-Sforza theories of that wonderfully interesting topic.

Jacobs' conclusion says: "On a purely empirical level, it has been shown that although long bone lengths remain little changed during the Dnieper Rapids Mesolithic-Neolithic transition, post cranial robusticity increases markedly in both sexes in the Neolithic. In contrast to previous discussions of increased Neolithic robusticity, . . . however, the view that this tendency reflects the in-migration of representatives of more primitive populations finds no support here. Instead, the robusticity increase is explained most plausibly as a reflection of increasingly stressful musculo-skeletal activity patterns on the part of the Neolithic individuals."

"Interpretive difficulties arise at the level of explaining the source and nature of the changing musculo-skeletal activity patterns. From the perspective of the currently prevailing archeological model for the Neolithization of the Ukraine (the 'out-of-Danubia' model), the robusticity increase in the Ukrainian Neolithic may be seen as the result of indigenous hunter-gathers having to work harder at traditional subsistence tasks. The need for increased effort would stem from demographic and other pressures emanating from expanding agropastoralist socio-economies rooted in south-central Europe."

"Alternatively, some recently accumulating data suggest that Ukrainian early Neolithic populations were enmeshed in a sedentary, grain-based subsistence both earlier and to a greater extent than previously thought. In addition to suggesting that the Neolithic robusticity increases reflect a serious change in subsistence activities, these data also point to the corridor between the Black and Caspian Seas as either a source of or a conduit for important influences that demographically, socioeconomically, and biologically trans-

formed the early Holocene Ukraine."

What he thinks are "data" are in fact a collation of hypotheses brought together to support his point. These and the importance of the whole matter to our knowledge of the Neolithic, the IE homeland, and strangely enough the classification of IE and West Caucasian, all require us to dwell more on his other remarks. The boundaries of Nostratic and Dene-Caucasic have also been breached, or so it might seem. We will want to peruse the bold paper by John Colarusso which is involved here too.

Jacobs had this to say, shortly before the conclusion: "From Upper and Epi-Paleolithic times onward, sites from the circum-Caucasus region and those from the zone encompassing the lower Dnieper River drainage and the Sea of Azov coast demonstrate numerous similarities in the archeotologies of their assemblages and in their ecological adaptations . . . Among the intriguing lithic similarities is the extremely early appearance of 'trapezoidal' armatures. Although dated to Late Pleistocene times in North Africa and southwestern Asia, around 8,500 B.P. is commonly accepted as the earliest date for their appearance in Europe (in the Balkans . . .). Yet the 10,000 B.P. 14C date for Vasylivka 3 . . . confirms their presence in the Pontic basin well before this time. It also firmly establishes a broader time span into which similar material from the Sea of Azov and eastern Black Sea regions — material previously dated tentatively at or near the Pleistocene/Holocene boundary on biostratigraphic grounds — can now be placed."

"Paleolinguistic data provide further evidence for extensive and intense interaction between the southern Russian Plain (including the Dnieper drainage) and the zone between the Black and Caspian Seas during the earliest Holocene. In a recent analysis (Colarusso 1992), a close phyletic link is proposed between the currently accepted reconstruction of the Proto-Indo-European language and that of the Proto-Northwest Caucasian language. On the basis of the consensus relative chronologies now in use in paleolinguistics, the time depth of the common ancestor linking the two languages, what has been termed 'Pontic', is said to be roughly 9,000-11,000 B.P. (Colarusso 1992:21)."

"This time frame brackets the date assigned to the Ukrainian Mesolithic sites reported here. It also represents a period during which crucial socioeconomic transitions were under way in the circum-Caucasus and in zones farther south-southwest. In the Caucasus region itself, indigenous and independent adoptions of sedentary food production may have occurred . . . In the Levant, the development of the Natufian and its related coeval/successor traditions in the Mediterranean and Irano-Turanian zones (Bar-Yosef and Belfer-Cohen 1989) may have had repercussions that passed through the inter-Black/Caspian Seas corridor and beyond. In this context, a recent morphological analysis of the dentitions from the Ukrainian Mesolithic cemeteries (Haeussler n.d.) is not without potential interest. This analysis shows that, in comparison with other Upper Paleolithic and Mesolithic material, the Ukrainian teeth demonstrate a marked and unusual lack of either western ("European") or eastern ("Mongoloid") traits. The implications of this for the broader population relations of the Ukrainian groups remain to be fully explored, but given the state of current knowledge, the existence of temporally deep yet

ongoing links between populations of the southern Russian Plain and those of the circum-Caucasus or even the Levant appears more than simply plausible (Haeussler, personal communication)." End of quoting.

A few of his many sources are cited here:

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A LINGUISTIC CONTRIBUTION FROM SOUTHEAST ASIA

Another lacuna in Ruhlen's *Guide* and Fleming's review article (in *Mother Tongue* 20). It seems much more important that the other lacunae, but it alone shows how fragile our sources of linguistic data and analysis are, being spread around the world in scores of separate, usually small journals, written in many different languages and scripts. Archeology suffers from the same problem to a lesser degree, while the biologicals hardly have a problem at all.

Paul Benedict wrote in late September (1993):

"Dear Hal,

I enclose an offprint of the Lai article. You don't even mention the language in your review, app. because you hadn't heard of it (a sound reason!), and Diffloth seems intent on burying it, for reasons I can hardly guess — he did indicate to me that he agreed with me that Lai is very divergent, and he also favors the Salween Basin as likely AA homeland. (AA in this case equals Austroasiatic — HF.)

It occurs to me that MT readership should at least know of this, and perhaps you might want to add a postscript to your article in MT-20. If so, please indicate that Diffloth does not go along with making Khasi as divergent as in my diagram on p. 21, and I wouldn't fight him on that — the position of Lai is the main point — and you should call it Bolyu, the autonym (Lai is Chinese term). Lai also is not to be confused with Hlai, the autonym now in use for Li, the Kadai language on Hainan!

In looking thru the paper, I realize there are several points of interest to long rangers that you might want to

include:

p. 1: How syllabicity, phonemic systems, and tones are all readily borrowed in SEA (= South East Asia — HF).

p.2 and my note 20 on Suprasegmentals paper: Loans can be very early and not mean that another genetic relationship is to be posited, here between Lai and TB (= Tibeto-Burman — HF), even though involving core verbs such as 'eat' and body parts such as 'head' and 'flesh'. Perhaps the info. on these early TB loans in Lai should be passed on to MT readership for possible parallels elsewhere.

p. 6: How older-than-Ego kin terms tend to be borrowed while younger-than-Ego terms tend to be retained. Important point!

pp. 7-8: How terms reflecting basic morphology can be borrowed.

... (other notes not included here — HF)

p. 22: Important for long rangers! Limited corpus > linguistic prediction (of a sort)!"

End of letter, end of quoting.

Another letter (9/29/93) expanded on Lai and reported on a conference in Osaka. (Only pertinent points are reported here and those not requiring the reading of other enclosed papers.)

"I also enclose a copy of another paper I gave there, on PTB / PST pronominals/pronominalization. As I start out with, AC (= Archaic Chinese — HF) is hard to figure! Fantastic dyschronicity in those pronominals and functors. Anything comparable elsewhere?

The big news from Osaka is the gridlock in Sinitic. The sinologists, for the most part now, depart from Karlgren and do all sorts of strange things in the recon(struction) of AC, ending up with a large array of non-cognates from the TB point of view. The *sk- ~ *sg- line of recon. that Bodman (in part) and I came up with in 1974, independently, makes good sense for TBists — see App. I and App. II of my paper. As you can see, it also gives all sorts of cognate networks within AC itself. The TBists, like Matisoff, are well aware of this but hesitate to dispute the sinologists, who operate in a world of their own, something like the old KGB in the Soviet [Union]. Not to brag but simply to state a fact, I'm the only one who has worked extensively in both the TB and AC fields. I keep telling Matisoff and the other TBists, 'Take my word for it', etc. and I talk about how AC has become infested with an *r virus (the sinologist stick in /r/s all over the place, which don't fit with the TB roots and hence have to be explained as 'infixes', a process unknown to TBists! . . . Non-TBists like Starostin have been led astray here, as one might expect, so that the recon. of PST now makes it quite inadvisable to make any use of it for wider, non-ST cfs (= comparisons — HF). . . . About recon. once again, Hal's CM-1 vs. CM-2 in MT-20:5. I see only one comparative method; the first step, of looking around to see which are the likeliest languages to be related is hardly another

method, as I see it. Even here one should always look at the earliest possible forms, e.g., when I started on Japanese as possibly related to AT (= Austro-Thai — HF) I paid especial attention to the Old Japanese and Ryukyuan forms, many conveniently put together by Martin. I came upon enough likely cognate sets that I was encouraged to study the matter in great detail. I would have *loved* having a PJR (= proto-Japanese-Ryukyuan — HF) to compare with my PAN (= proto-Austronesian — HF), PKD (= proto-Kadai — HF) and PMY (= proto-Miao-Yao — HF) but found that none had been done — this still true, unfortunately. But note how the early forms make my paper on Japanese/AT clan ... possible — if we had only Modern Japanese forms, none of this would be possible. I think it all can be put in a single rule: always use the earliest possible forms; even in isolates, internal reconstruction is often feasible to some degree. For example ... (discussion of a test case to confront differing classifications) ... I think that Allan has done a good job in getting out MT-20, with the help of Hal's great review of classifications, including the isolates that Ruhlen neglected. I've looked at Kusunda and found nothing to remind me of ST, hence I am surprised to hear that Ruhlen is working on that angle for the language. All the KDists now use /Kadai/, not /Daic/, so the latter should be dropped. The 'newest' MK (= Mon-Khmer — HF): Lai, better now the autonym: Bolyu, spoken in Guangxi by very small group, surrounded by a mess of other languages, paralleling Vietnamese in being monosyllabic, with a complex tonal system, with strange ancient loans from TB along with anticipated later loans from KD and Chinese, described by me in *LTBA* 13:2: 1-24, 1990 ('How to tell Lai': — I couldn't resist using the Chinese term for this title!); I posit possible earliest branch (!) from PMK bloc; Diffloth has indicated he's not opposed but he has neglected even to mention Lai in his latest papers! No idea why. Latest from Diffloth: on MK, a plain vs. glottal/creaky prosody must be reconstructed for PMK; on Austric, he agrees with me that there is no substantial shared lexicon between AN and AT (sic! He surely meant AA not AN — HF)! At the Hawaiian conference last May, he and others emphasized the morphological correspondences — but said little about lack of suffixes in AA, a key divergence! Sorry, Hal, but there's a big lexical black hole where Austric sits, and you have Gérard and me agreeing on this! One more point about Bolyu: Jerry Edmondson (Texas) has recorded the language, and much more material is now available, so I'll do a follow-up when I get time. Right now, [I'm] trying to do a 'definitive' recon. of PMY, working with Martha Ratliff (Wayne State), who is into computers (I'm not yet but planning to), and planning things like updating my AT and getting out a handbook of ST, along with collaborating on etymological dictionaries of both Chinese and Japanese — absolutely nothing of value for either now (exists — HF). . ." End of quoting.

It may be of interest to our historians of science that Paul Benedict is one of the rare living linguists who studied directly with Edward Sapir. Greenberg and others were influenced by Sapir, but is there anyone else in our ranks who studied with Sapir? Yes, Susan Park (Carson City, Nevada) at Yale in the 1930s!

“BOLYU” OR “LAI”: A NEW BRANCH OF MON-KHMER, FOUND IN CHINA!

In 1984, a Chinese linguist named M. Liang discovered a language spoken by several hundred people, living in the southeastern part of China known as Guangxi (modern spelling). Their own name for their language is “Bolyu”; this was determined more recently after Liang called them by their Chinese name of “Lai.” He found that their traditional homeland was in the west, in southwest Guizhou and southern Yunnan, where Lai had been replaced by Lolo (Chinese Yi) a Tibeto-Burman language.

As mentioned above, Paul Benedict wrote “How to Tell Lai: An Exercise in Classification,” a terrible pun but hard to forget. The article is recommended to you; it's in *LTBA* 13/2:1-26. What we report here are three things, viz., its areal phonology, some of the lexicon, and Benedict's bold classification.

Bolyu has a standard Kadai typology (Kam-Sui type), having: monosyllabic morphemes, limitation of non-vocalic finals to glides (written *-i, -u*) and *-p, -t, -k, -m, -n, -ŋ*: six tones (only five before stop finals), aspirated vs. unaspirated surd stop initials, with separate postvelar series: apical affricate series along with /b/ and /d/ (both prenasalized). The eight vowels (in native words) show a simple length distinction for /u/, /i/, /e/, and /ə/ (schwa — HF) and a highly idiosyncratic three-way length distinction for /a/, with both long /a:/ and half-long /aː/: the word order is VO, and in noun compounds the “possessor” normally follows [suk¹ nu¹ ‘hair, head’] but occasionally precedes [tôk¹ suk¹ ‘mouth-hair’ = ‘beard’], the latter probably through Chinese influence.

The six tones are numbered, as follows: /1/ high-level: /2/ mid-level: /3/ low-level: /4/ high-falling: /5/ low-falling: /6/ low-rising (lacking in forms with stop final).

Liang also “notes, however, that the lexicon reveals few similarities with Kam-Tai, Gelao (KD language of China), Miao-Yao or Yi, and he attributes the Austroasiatic features in the numeral system to contacts with Vietnamese or other languages of that stock. He concludes his remarks with the observation that the affiliations of the language constitute a ‘complex and richly interesting problem’.”

Benedict agrees that it is “indeed an intriguing problem, yet an answer appears to lie at hand, even on the basis of the limited available material in this sketch of the language. To begin with, one can quickly dispose of both Sino-Tibetan and Austro-Tai as possible congeners, on the basis of the lack of any substantial amount of shared basic vocabulary. As can be expected, Lai is well stocked with loans from Chinese, including those for the numerals above ‘1,000’, but none from basic vocabulary. Rather surprising, however, are the apparent early loans from Tibeto-Burman including those for some ‘non-culture’ items.”

Hereinafter, we will present Bolyu data by clusters as they are seen relating to outside groups by Benedict. For more detail, one must refer to the original article.

Group of early loans from Tibeto-Burman. Note: we of *Mother Tongue* neither endorse nor reject these hypotheses;

we merely report them.

to:ŋ ¹	thousand
žō ²	hundred
qō ¹	sky/rain
la:i ⁶	ox/cattle
kye ⁴	fowl
bu ¹	head
ka:p ⁵	needle
qa:t ⁵	run

Benedict notes being "struck by the fact that these loans as a whole relate to TB generally rather than to Burmese-Lolo languages in the vicinity of the Lai, with forms often mirroring reconstructed PTB roots rather than the much modified forms of languages such as Loloish. It appears that some of these loans, at any rate, were made at an early period, when the ancestral Lai were located well to the west of the Lai of the present day."

Group of loans from Austro-Tai, more explicitly from Kadai, most specifically from Tai and/or Kam-Sui.

khvân ⁵	ax	vi ¹	firewood
va:i ⁶	cotton	qo:k ⁵	animal enclosure
kau ¹	horn	fi:p	fish scales
ʔa:u ¹	uncle (FYB)	kal lye ²	child
le ² da:i ¹	inside	le ²	(a locative)
ŋam ⁶	dumb(mute)	muō ² / ma	come
ʔōk ¹	go out	ha:i ⁵	open (door)

Benedict notes that this is "a sizable number of loans, to be sure, especially in view of the limited amount of material available, but the Lai inhabit the very heart of the NT (Northern Tai — HF) homeland, with KS (Kam-Sui — HF) speakers also in the region, hence this borrowing should hardly come as a surprise."

Group of forms which appear to relate as cognates to Mon-Khmer forms. The first five ('water' through 'hair') seem cognate with "highly persistent" Mon-Khmer roots.

de ⁴	water	qō ⁴	fish
tsu ⁴	dog	ma:t ²	eye
suk ¹	hair	han ⁶	day
san ⁴	bird	mi ³	fly
			(noun)
lyiŋ ⁶	horse	kai ¹ lye ²	child
			(< Kadai)
nam ⁶	year	mau ³	stone
muō ⁵ lya:ŋ ⁵	eagle	qa:m ⁴	husk
			(of grain)
ñō ²	house	ʔau ²	deep
qhōŋ	mountain	mya:n ⁶	salt
be ⁴	rice (early)	tham ⁶	egg
kō ⁴	road	lân ²	long
žu ²	go	hyō ⁴	rob
mōŋ ⁶	listen, hear	mi ⁶	laugh

This is also true of the primary numbers from 1-10. Unless they are all borrowed, they make an imposing set. Hal checked these against Munda (AA of India) numbers, where they did almost as well:

ma:i ⁵ / mâ ²	one	piu ⁴	six
bi ¹	two	pai ¹	seven
pa:i ¹	three	sa:m ⁴	eight
pu:n ⁴	four	šan ⁴	nine
me ⁵	five	ma:n ²	ten

Number five is said to be "innovative" and not part of this set of cognates with Mon-Khmer. (It bears a small resemblance to Munda forms in *man-* and *manul-*, but hardly convincingly — HF.)

Benedict finds the pronouns also "independent," i.e., independent of any one Mon-Khmer cluster, but still affiliated with Mon-Khmer. Without further comment they are:

ʔa:u ¹	1st sg. (I)	ʔa:i ¹	1st pl. (incl.)
		pa:i ¹	1st pl. (excl.)
mi ²	2nd sg. (thou)	ma:i ²	2nd pl. (you)
ʔi ¹	3rd sg. (s/he)	cōŋ ¹	3rd pl. (they)

(We are unable to say what the value of /c/ is, whether [ts] or [k] or [č] is unclear. As usual, "c" is an international problem, due to each wee fiefdom in historical linguistics doing things its own way, following the rules of some European language. So are "j" and consonants written with dots under them — HF.)

Other terms are embedded in Benedict's text of rich discussion of possible cognations. We leave them to the reader to probe on his/her own. More data are coming out from the recordings of Jerry Edmondson. The main points, but one, have been made.

What does Paul Benedict do with Bolyu taxonomically? "We do now, after this review of all the available material, have an answer to the question: what is Lai? It is at best a MK language but has been 'Kadaicized': reduced largely to monosyllabic forms, thus stripping it of virtually all of its morphology, and provided with a complex tonal system. In all of this, it closely resembles Vietnamese, with a specific parallelism in the tonal development, inasmuch as initial voicing and final glottalization are prominent conditioning factors in both cases. Each language, in addition, has a large corpus of loans from Tai, but Lai has added a further complication: a body of loans from TB. Maspero, a pioneer in the Southeast Asian field, pronounced Vietnamese to be a congener of Tai: one can only wonder what he would have done with Lai."

What is Benedict's final proposal for Bolyu? We show it in a different way than the chart he drew on p. 21 of his paper.

Mon-Khmer	>	Khasi + proper MK
proper MK	>	Bolyo + narrow MK
narrow MK	>	all the other MK languages, but Bolyu and Khasi

This classification is not followed by Diffloth, Benedict reports. That is normal for Austroasiatic's internal taxonomy. For the sake of showing how far apart the might be, we give Diffloth's 1982 overall Austroasiatic classification, thus:

AA	>	Munda + Mon-Khmer
MK	>	North + East + South
North MK	>	Khasi + PALAUNGIC-KHMUIC + VIET-MUONG
East MK	>	KATUIC + BAHNARIC + Khmer + PEARIC
South MK	>	MONIC + ASLIAN + NICOBAR ISLANDS

Where does Diffloth put Bolyu in this scheme? We wish we knew.

MORE FROM BIOGENETICS

(The gist of this was reported in *Nature*, 17 November, 1993, And the *New York Times* (p. A10) the same date. The rest is by Hal Fleming.)

1) *Australopithecus afarensis* (A.a.), or basically "Lucy" and her tribe, were not all midgets. *Sexual dimorphism* prevailed. She who most of the modern world has accepted as the critical ancestor of human descent lines (hominids) was *très petite*; Lucy was probably not more than three and a half feet high (= ca. 105 cm tall). Most adult African pygmies range from four feet to four feet eight inches or 120 cm to 140 cm. Modern African women are usually more than 155 cm, while some Nilotes, some Tutsi, and some professional basketball players (all adult males) are twice as tall as Lucy (ca. 210 cm) and more. Interestingly enough, while the pygmy chimpanzee or "bonobo" (*Pan paniscus*) is somewhat shorter and slighter, adult common (regular, "proper") Chimpanzees stand about five feet (150 cm) and four feet (120 cm) tall, males and females respectively. Bonobo males weigh around 100 lbs (7 stone and 2 lbs) and females around 70 (ca. 32 kg). Proper chimpanzee males have been recorded as heavy as 175 lbs (ca. 80 kg).

Modern human males generally are said to be 15% to 20% heavier than modern women, due to more bone and muscle mass, and 5% to 12% taller. Between ethnic and regional populations, there are considerable differences in height and weight too. But as Franz Boas showed long ago, these difference are not all due to inheritance, since some groups change their sizes as they change their diets. Modern studies have tended to confirm the Boasine theory, e.g., most recently the Japanese have increased their size considerably — the ancient Chinese called them "dwarfs".

Thus, modern *Homo Sapiens Sapiens* is a markedly polymorphic species from region to region and usually dimorphic between the sexes. What had been thought to be an ancestral hominid population of tiny folk, which some

imagined was much akin to the pygmy Chimpanzee, turns out to be a mistake due to ancestral dimorphism. The men in Lucy's tribe were quite a bit taller and heavier than Lucy. Some were a foot taller (or 30 cm) than Lucy, putting them well within the range of modern African pygmies and male bonobos. Of course, there are still many things about the bonobo which look more humanoid, e.g., tendency to stand up frequently, face to face copulation, etc.

These considerations have also laid to rest speculation that there might be *two species* of *Australopithecus afarensis* in Ethiopia because of the larger males. Now, due to the recent research done in the Awash basin of Ethiopia (near Maka about 80 km west of Hadar) by Tim White (Berkeley) and his team, we can see that Lucy still has the same taxonomic position as before. Her tribe just had larger males than females. The dates of 3.4 mya are still in place, and the affinity with the Laetoli foot prints in Tanzania of 3.7 mya show that A.a. was walking upright 4,000,000 years ago.

Would someone like to write for us a summary of reasons for believing that Lucy was more like a bonobo than a proper chimp?

"The Genetic Structure of Ancient Human Populations," by Henry C. Harpending, Stephen T. Sherry, Alan R. Rogers, and Mark Stoneking, in *Current Anthropology*, Volume 34, Number 4, August-October 1993, pp. 483-496. All but one of the authors are in the Department of Anthropology, Pennsylvania State University, University Park, PA 16802, USA. Alan R. Rogers is at the University of Utah. Malheureusement, there is no abstract.

The text of this highly significant paper is only 13 pages long, but at times it is neuron-befuddling in its technicality. In the face of the complexities of population genetic theory, mathematical models, computer simulations and an off-putting specialized vocabulary, verging on gobbledygook, it is a serious chore to report the paper's message to an audience that only speaks English. The only things harder to understand than this have been Tony Traill's acoustic analysis of Khoisan clicks and some proto-Caucasic reconstructions. Either of these give one almost as much psychic distress as trying to locate a book at the British Museum, or so they say. Or reading a computer manual, after the beast has destroyed an entire day's work.

Do you suppose that the emerging synthesis will be cut off at the impasse by the private jargons of the sub-fields?

Yet Harpending et al. have some important things to say in clarifying the aging debate on mtDNA and its usefulness in reconstructing genetic prehistory. I will try to grasp the main points and translate them for you. The really hard stuff which baffled the brain will not be reported. We will report the first three paragraphs in toto, a few remarks in between (where all the hard stuff is concentrated), and then their conclusions. You will see that *their paper is well worth struggling with*.

We quote (p. 483-484): "Differences among human mitochondrial DNA (mtDNA) sequences are an important source of data about the history of our species. Since mtDNA sequences are not broken and reformed by recombination, they are tips of a tree of descent. There are several approaches to using mtDNA sequences to infer properties of the tree of descent and relate those properties to the history of the

population in which the tree is embedded."

"The direct approach to inferring properties of the tree is to compute a reconstruction of it using one of a number of algorithms that make trees from differences among objects. Cann, Stoneking, and Wilson (1987) used a maximum-parsimony algorithm to reconstruct the tree of descent of a sample of mtDNA from many different human groups, and they were led to suggest that our mtDNAs all descend from an African who lived approximately 200,000 years ago. Although Vigilant et al. (1991) have supported this result, it has been subject to a number of criticisms, the most important of which is that current methods do not reliably reconstruct the tree (Hedges et al. 1992, Templeton 1992, Maddison, Ruvolo, and Swofford 1992)."

"The task of relating properties of the tree to properties of the population has not been handled carefully in much of the literature, especially in the commentary that followed the original work in the popular press. Some authors have assumed that the coalescence at 200,000 years implied that a new population arose at that time, but the genetics suggests no such thing. The age of the coalescent (the common ancestor) reflects population size in the past, in this case suggesting that the effective number of females in the late Middle Pleistocene was on the order of 1,000 to 10,000. It is not related in any simple way to population origins. In fact, the concept of the origin of a population is not clear, but it seems to mean growth from a small partially isolated subpopulation of the parent species. In these terms, we can distinguish three models of the origin of modern humans. The *strong Garden of Eden hypothesis* posits that modern humans appeared in a subpopulation of *Homo Erectus*, perhaps as a new species, and spread continuously over much of the Old World. The *weak Garden of Eden hypothesis* posits again that modern humans appeared in a subpopulation and spread slowly over several tens of thousands of years, then later expanded from separated daughter populations bearing modern technologies such as those of the African Late Stone Age or the European Upper Paleolithic. The *multiregional hypothesis* posits that the entire *H. Erectus* gene pool contributed to the gene pool of modern humans."

"In this paper, we use a new method of analyzing mtDNA sequences that is based on a theory of how *mismatch distributions* — histograms of the number of pair-wise differences in a sample of DNA sequences (Hartl and Clark 1989) — should preserve a record of population expansions and separations in the remote past (Rogers and Harpending 1992). We use 'mismatch distribution' here to refer to differences among sequences within a population, and we call mismatch distributions between sequences from two different populations *intermatch* distributions. (Elsewhere in the literature, mismatch distributions have been called *distributions of pairwise differences*.) Human mtDNA mismatch distributions preserve a record of past population dynamics. We show that they are incompatible with the strong Garden of Eden hypothesis, marginally compatible with the multiregional hypothesis, and easily compatible with the weak Garden of Eden hypothesis. Other genetic and ecological evidence, however, denies the multiregional hypothesis unless there was a marked worldwide population bottleneck within the past

200,000 years that reduced the total size of the species to a few thousand *without* leading to extinction of regional subpopulations. Mismatch distributions are also incompatible with the hypothesis that the relatively recent common ancestry of human mtDNA reflects replacement by a new selectively advantageous variant." We stop quoting.

We mention their population samples (pp. 485, 487) for your information and because it is normally the point where biogenetics gets sloppy.

For "RFLP" (restriction fragment length polymorphism) they collated from the literature (much of it reported previously in *Mother Tongue*), they sampled 21 Australians, 20 African-1, 34 Asian-2, 47 European-2, and 119 Papua New Guinea-2.

For "HVS 1" (hypervariable segment from region 1 of mtDNA), they sampled 34 Asians, 41 Bantu-speakers, 42 Middle East, 39 Herero, 64 !Kung, and 63 Nuu Chad Nulth. They apparently did not know that the Herero speak Bantu, that half of the Middle East is in Asia, and that the Nuu Chah Nulth are usually called the Nootka.

For HVS 1/2, they sampled 41 Bantu-speakers, 32 Papua New Guinea-1, 24 Asian-1, 20 European-1, and the same 64 !Kung.

Since most of the Asians in their samples, judging by the sources mentioned, are East or Southeast Asians, it is likely that India and Siberia are once more unrepresented in a world sample. Since they appear to treat the Herero separately from the other Bantu, no doubt the diversity in Africa is again misrepresented. This is particularly unfortunate because the Herero are very distinctive, being connected by language and culture to two different clusters of Bantus, roughly western and eastern, and by physique possibly to East Africans and/or "Hamites". But their choice of the Nootka is doubly misleading because great scoops of mtDNA data are available on Amerinds, but the Nootka stand at the interface of seriously different human groups, the Na-Dene and the Amerinds. Gene flow maximized.

Nevertheless, despite these carping comments which our colleagues in biogenetics never seem to take seriously, it does not or *probably does not* affect their general conclusions. Just too much of global diversity is represented for them to be far off the mark. Their conclusions are interesting indeed.

A small one on p. 493 en route to the mains ones:

"*Younger and older populations*. Figure 6, ...illustrates the often-repeated finding that African populations have more sequence diversity than European ones. The difference between Africa and Europe is particularly marked in this set of data, but we have seen the pattern in our other sets of typings. The obvious suggestion from the data is that the African population is roughly twice as old as the European; that is, Africans seem to have expanded 100,000 years ago, while the Europeans expand only about 50,000 years ago. The intermediate position of the intermatch distribution suggests an absence of prior separation between the ancestors of these groups — the ancestors of Europeans seem to have grown from a subpopulation of the ancestors of Africans. Are these reliable inferences?... (After a long technical discussion)... These

results show that the ancestors of Europeans did indeed undergo an expansion much later than the ancestors of Africans."

Then their main conclusions begin at the end of p. 493.

"Our qualitative conclusions depend on the robustness of our simulation results, and our quantitative conclusions depend on the validity of current estimates of the mutation rate for mitochondrial DNA. We have shown simulation results for samples of 40 that are based on a common scenario of expansion five mutational time units in the past. We have experimented at length with other scenarios and have found the same features that we report here. There are, however, many complex possible histories of subdivision, population growth, and bottlenecks, and we can hope to understand the consequences of different histories only in the most general way."

"Estimates of the mutation rate of most mtDNA have been derived from the differences between human and chimpanzee DNA and from diversity within isolated populations whose time of origin is known archeologically ... While both methods have their problems, they do provide estimates that are remarkably consistent with each other. Errors in mutation rate estimates would translate proportionally into errors in our time estimates; for example, if current methods have underestimated mtDNA mutation rates by a factor of one-half, then our data would suggest expansions of about 30,000 years ago rather than 60,000."

"Given these caveats, our results show that human populations are derived from separate ancestral populations that were relatively isolated from each other before 50,000 years ago. Major population expansion took place between 80,000 and 30,000 years ago — 80,000 years ago in Africa and perhaps 40,000 years ago among the ancestors of Europeans. How long were these ancestral populations isolated from each other? Since all human mtDNAs coalesce approximately 200,000 years ago, mtDNA provides no information about population structure before that time. The mtDNA coalescence time is consistent with indications from other polymorphisms that the ancient effective size of our species or its ancestral species was of the order of 10^3 to 10^4 (to the 4th power — Ed.) females. (Ed. > 1000 to 10,000 females). . . This rather small effective size seems difficult to reconcile with our evidence of isolated, hence geographically separated, ancestral populations."

"Could *H. erectus* or early *H. sapiens* have occupied large parts of Africa, Europe, and Asia yet have consisted of only several thousands or tens of thousands of individuals? Clark (1968:149) estimates that of the approximately 48 million km² of arable land in the Old World, 20 million km² are in cold-climate areas and 'two-crop tropical areas.' Since *H. erectus* did not use these areas, the land occupied by these species in the Old World may have amounted to roughly 25 million km². Lee and DeVore (1968), in their summary of a conference on foraging peoples, suggest that reliable reports of contemporary density are 1-25 persons per 100 sq. mi. Upper Paleolithic population densities in Europe were much greater than those of their archaic predecessors. The middle of the Lee and DeVore estimate is about 5 persons per 100 km². Reducing this by one order of magnitude, gives 5 per 1,000 km² or

125,000 as a world population of *H. erectus*. Weiss (1984) arrives at an estimate of 500,000 by positing a small occupied region of the Old World but densities equal to those of contemporary foraging populations. A total population of 125,000 would correspond to an effective number of females of roughly 25,000. The effective population size of a fluctuating population is much closer to the minimum than to the average, so the ecological estimate of 25,000 is not wildly inconsistent with high-end genetic estimates of the long-term effective number of females 5,000. It is, however, on the edge of credibility because our ecological assumptions favor a small world population of *H. erectus*, and the mtDNA data suggest that the ancestral effective size was closer to 500 females."

"Comparisons of Acheulian assemblages over large areas with assemblages left by modern humans reveal a remarkable uniformity in the Acheulian from the Cape to Korea (Isaac 1972; R. Klein and S. Ambrose, personal communication). Even derived artifact industries such as European Middle Paleolithic (or Mousterian) are characterized by remarkably little variability through time and space. The number of readily identifiable stone tool types they contain is relatively small, and formal bone artifacts, decorative items, and art objects are all but absent. It is hard to imagine how this apparent cultural uniformity could have persisted without high levels of movement and mate exchange between groups. This mate exchange ought to have maintained unity of the species or at least the macro population. If local groups were bands of 25, there would have been 5,000 bands occupying 25 million km² or 5,000 km² per band. If these had been packed into a hexagonal lattice, the average spacing between bands would have been about 75 km; because instead they would have been concentrated around special features of the environment such as river valleys, the real average spacing might have been considerably less, perhaps within the range of dispersal of a large mammal. Arguments from the homogeneity of cultural remains of *H. erectus* must also contend with the apparent ancient separation of the world into the Acheulian region and the Asian chopper-chopping tool region (Coon 1965). This cultural evidence of ancient separation is compatible in an interesting way with our evidence from intermatch distributions of at least two ancient populations that were extremely isolated from each other before the population expansion of the Upper Pleistocene.

"The ecological estimates that we have derived are too rough to be taken very seriously. Population densities in most species vary widely. Extrapolation from densities of contemporary foragers is much more compatible with contemporary human ancestry concentrated in a population of several thousand in, say, Africa. While population genetics so far cannot absolutely distinguish between the Coon-Weidenreich or multiregional hypothesis (Wolpoff 1989) and the Garden of Eden hypothesis of the origin of modern humans from *H. erectus*, it strongly favors the latter. The uniformity of the Acheulian suggests high levels of migration between groups, but the small effective size of the total population of modern human ancestors suggests that they could not have been spread over such a large area and still maintained high rates of gene flow. Even with assumptions that radically favor the multiregional hypothesis, we arrive at total population size

estimates of *H. erectus* that are too large to accommodate the genetic data."

"A scenario that is consistent with both the ecological and the genetic lines of evidence is as follows: Around 100,000 years ago, ancestral humans spread into separate regions from a restricted source, but there was not necessarily a dramatic expansion. The data are consistent with an early expansion and subsequent bottlenecks or with an early modest growth and slow expansion. Later, starting around 50,000 years ago, dramatic population growth and expansion occurred separately within dispersed daughter populations that were genetically isolated from each other. This is the weak Garden of Eden hypothesis. In the archeological record, the time range of 45,000-35,000 years ago is an approximate boundary for an apparently dramatic change in human behavior. In the interval between 50,000 and 40,000 years b.p., the Mousterian, the African Middle Stone Age, and similar industries were widely replaced by clearly more advanced industries, including those of the Upper Paleolithic, in which the degree of geographic and temporal variability is far greater, the number of readily recognizable stone artifact types is much larger, and formal bone artifacts and art objects are common (Klein, personal communication). The suggestion is that culture rather than biology drove the burst of growth of our ancestors. This scenario is also consistent with evidence from protein polymorphisms (Net and Roychoudhury 1993) and from nuclear DNA (Mountain et al. 1992)."

"The present data are clearly inconsistent with the strong Garden of Eden hypothesis. If there was indeed a single large expansion from Africa around 100,000 years ago, we should see the signature of it in the mtDNA differences, but instead we see indications of multiple later expansions associated with modern technology instead of modern morphology. Another version of the strong Garden of Eden hypothesis posits a selectively advantageous new mitochondrion that swept through an extant population. This selection hypothesis would leave a wave identical to one generated by population growth, but we see evidence of not one wave but many. If a new advantageous mutant had swept through the population, then the growth in frequency of this mutant would have had the same effects as the population growth that we have identified as the cause of the observed waves, but in this case the intermatch distribution would *not* lead the mismatch distributions because prior separation would not affect the accumulation of differences in descendants of the new advantageous mitochondrion. The existence of between-group differences far older than within-group differences implies that the late Pleistocene expansion of our species occurred separately in populations that had been isolated from each for several tens of thousands of years." Here ends the quoting.

A small selection of sources mentioned in our quotes is given; the criterion is your probable familiarity with it. The R. Klein mentioned in the text is Richard Klein, formerly of Chicago, now of Stanford University. S. Ambrose is Stanley Ambrose, sometime long ranger, who is at University of Illinois.

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A REVIEW OR REHEARSAL OF KEY POINTS PLUS COMMENTS ON THEIR MEANING

This is important enough to bother with rehearsing what has been learned. Then on to consider what it portends. Here's a list:

- 1) An African homeland for H.s.s. (*Homo Sapiens Sapiens*) is not disputed but not highlighted either.
- 2) African diversity is again seen as greater than that of any other world region. However, out of respect to Alvah Hicks, one must repeat that diversity in the Americas was not tested.
- 3) A European homeland for H.s.s. is implicitly rejected, since Europeans come from ancestral Africans.
- 4) The theoretical ancestor/ancestress — Eve — of 200 kya has been downgraded and branded as unlikely genetically.
- 5) A starting time of 100 kya (100,000 years ago or 100 kilo years ago) has been postulated as the start of H.s.s. on our trip to settle the rest of the world *and* the rest of Africa.
- 6) H.s.s. did not occupy the whole Old World in one fell swoop, not in one grand migration, not necessarily by conquest.
- 7) After H.s.s. left Africa, we settled in a number of regions first, *isolated and separated*, before going on to occupy

the rest of the world. Or in linguistic ways of thinking, we went from proto-human to proto-region-A, proto-region-B, etc., etc.

8) After living in those key regions from 20k to 50k years, we underwent regional explosions or rapid growths in the *cultural* sphere (that includes *technology*), becoming the contemporary counterparts of the European Upper Paleolithic in various regions of the Old World.

9) Around 50 kya plus or minus 10,000 years, these dormant clusters of H.s.s. expanded from their havens or ecological prisons or nesting places and completed their occupancy of their regions.

10) The natural questions of where those nesting places were are not answered at all. I doubt that Harpending et al. found them uninteresting but rather they were beyond the scope of the paper.

A close reader of the paper could surely find more key points to rehearse. But for our more general purposes, it is better to look at the larger or farther meanings of these points.

First, saying that Africa is homeland for anything is always a bit misleading and actually vague. It depends on what is meant by Africa. Many people clearly mean "sub-Saharan" Africa and often really mean central Africa of the Congo basin plus east Africa. In the Nile basin, of course, the whole concept of "sub-Saharan" is silly because the Nile connects everything from the Delta to Kampala and Addis Ababa. Moreover, Africa is such a large place that parts of it are very far from each other. Nairobi, for example, is as close to Tehran as it is to Cape Town. And Beijing is as close to Baghdad, as Nairobi is to Dakar.

Second, African diversity is not necessarily decisive in and of itself, despite Johanna Nichols's stress on sheer diversity as a criterion of age. For example, Austronesian seems to match Indo-Pacific or Niger-Congo in diversity, yet few taxonomists would think it anywhere near as old as the other two. African populations also are farther from European or Asiatic populations than those are from each other. To put it differently, what we might call the "node" or coalescent of Asian mtDNA is closer to the European node than either node is to the African node. Or proto-Asian is closer to proto-European than either is to proto-African. Sorry to repeat what we have said many times before.

Third, and more interesting, is that genetic support for two notions involving *Homo Sapiens neanderthalensis* or *Homo neanderthalensis* has now dwindled to nearly zero. Modern Europeans have an ancestor who was one of the old Africans. Hence, first, Europeans cannot be descended from neanderthal and, second, H.s.s. is not descended from neanderthal either. Although there are not too many adherents to the latter view, there are plenty who support the first view. Actually, the view taken by Jean Auel in her novels seems to be close to the truth. Modern people, some of them African, replaced neanderthals in Europe but without this really meaning that neanderthal was a brute or stupid or unfeeling or the like.

Neanderthals probably interbred with these moderns too, at least in Jean Auel's novels, but we do not as yet know for sure whether it actually happened. It seems quite likely

considering how sexy our species is but whatever genetic impact there might have been on modern Europeans has not yet been detected in the mtDNA.

Fourth, while Eve is lost, what replaces her as a concept is downright murky. Maybe someone can enchant us with the mysteries of population genetic theory that replace the Eve concept.

Fifth, no one will be upset with a date of 100,000 for the great African Diaspora, actually the second African Diaspora (*Homo erectus* was the first), because it is much more in accord with fossil evidence of "anatomically modern man" and does not break the hearts of linguists who wonder how they could ever reconstruct anything as old as 200,000 years.

Sixth, that the emigrant Africans — presumed here to be the speakers of proto-human — did not spread out swiftly to occupy the whole Old World (or that plus the Americas) and did not settle down to generate local dialects all over the world probably comes as no great surprise to linguists. Language is not as evenly dispersed as that in fact but rather comes in clumps.

How early H.s.s. spread out, how they treated their distant cousins they found along the way (at least in some places), and whether there was war or conflict or whatever is at the moment simply too hard to determine. That is up to archeology surely.

Seventh and eighth, the secondary regions, isolated and separated, are very riveting. Put a population in a limited area and isolate it for 20,000 or 50,000 years and what do you expect will happen to it? It will alter itself culturally, socially, and physically. It will adapt in depth to a new environment with all its fauna and flora and climate. The language will change into something else. Differences greater than those between proto-IE and London cockney will occur. Indeed Australia could be one model of what can occur. The huge difference between Tiwi and Aranda, for example, has taken thousands of years of separation, although not true isolation.

More to the point, just suppose that H.s.s. left Africa with only the basic inclination to speak but without a full-fledged language (proto-human) yet. In those cases of isolation, then, one may presume that regional equivalents to proto-human would or could develop. In that case, what we would see in the modern world would be a series of regional super-phyla which did *not* have a *common ancestor*, except in the "hardware" in the predisposition to speak, to label things, to make sentences, etc. The lexicon and morphology of each would be unique or at least unrelatable to the others. We would have true *polygenesis*.

Could such a thing be possible? Well, the ability or the lack of ability to compile convincing global etymologies is a pretty good test of polygenesis. We ought to give those scandalously "incorrect" studies a fighting chance. If we follow the dictates of the nay-sayers among us, we will *never know*.

In any case, an enormous amount of study and conjecture over the last, say, 30 years has asserted that having a human language in one's head makes a great deal of difference to one's thinking process. Languages were growing and contributing basic things to the emerging cultures of the regions. Given the bent towards materialism among many prehistorians, such a reminder may be necessary before

everything is explained in terms of calories and climates.

Again, these regional places ought to be seen as the nesting places of *races*. Granted that the concept of race has been abused in the modern world and granted that it is often an inefficient way to describe global physical differences, it may still be useful to label the products of the regional isolates as races. It may be that such a concept as Caucasoid or Mongoloid, granting also that these are close to being ideal types, may sum up nicely the physical attributes of the people emerging from one of the regional nesting places. This may be what the relatively weak correlations between linguistic phyla and biogenetic clusters are all about. I must grant that some of the ideas here are borrowed from the late Carleton Coon, whose name became unpopular among anthropologists in the 1970s and 1980s — due to alleged racism.

Ninth, the expansions of the regional foci of H.s.s. completed the peopling of the modern world, up to 1492 AD. If the regional theory does not include the settling of Australia and New Guinea and the New World, plus the invasion of Neanderthal's Europe, then the theory is of little use to us. It is probably not an accident that the current archeological dates of Australia plus New Guinea are around 55 kya and that the demise of Neanderthal begins around 45 kya and the colonization of North America is now more frequently seen as 20-40 kya by many people. Moreover, the internal dates — biogenetically — of Australians is close to 50 kya and so is that of Papuans, suggesting that their separations from each other began fairly soon after they moved to greater Australia.

In this context, it is probably the case that Colin Renfrew's theory of agricultural dispersals has been falsified. A date of 50,000 years ago for European expansion is just way too early for any theory of agricultural dispersals to be relevant. For those linguists who do not know the dates, first agriculture (Neolithic) in the Near East (hence in the world) is never cited as older than 10,000 years ago. Good colleague Renfrew is invited to challenge that point herein, if he wishes. Without debate. Just to challenge freely. Also, since we have never managed to squeeze his valuable hypotheses into the pages of *Mother Tongue*, he is also invited to present them to us. If I gave a summary now, everyone would suspect me of bias!

The tenth and intriguing point is: where were the regional nesting places (staging areas)? For various reasons, we know some of the likely ones already. Let us list each with its rationale.

1) Greater Israel or the Levant or southeastern Turkey. First, because Israel has a H.s.s. fossil circa 100 kya (Qafzeh); next because it is the so-called Caucasoid region we are looking at here and this area is close to the center of distribution of Caucasoids; next, because it is close to the interface between H.s.s. and neanderthals; next, because H.s.s. basically was documented living here for upwards of 55,000 years before we moved into adjacent Europe. (Why? Cf. Qafzeh at 100 kya \pm 10k minus 45k for probable earliest Upper Paleolithic in eastern Europe). Next, as oldest documented case of H.s.s. outside of Africa, it is next to Africa in a strict sense and just "down the Nile" from Ethiopia and East Africa, the most likely areas for the first origins of H.s.s.

2) Indonesia or eastern Sundaland. Nesting place for the so-called-Australoids, basically the Papuans, the native Australians and the alleged sub-strata in many places all the way back to India. First, and perhaps the only major reason, is that this is a geographical given. In this case, the expansion reveals the origin point. The very early settlement of New Guinea and Australia could hardly have come from anywhere else, and even that had to involve at least paddling, rowing, sailing, or swimming to cross open seas between places like Timor or Halmahera on the Indonesian side and New Guinea or Australia on the open Pacific side. Next, the two great linguistic super-phyla or very old phyla, centered on Australia and New Guinea, are *not* considered related to each other, except only as implicitly linked in proto-human, as attested in global etymologies. Yet, each is truly indigenous in its area, having no serious competitors for the role of autochthone. By inference, by virtue of the fact that both are derived from a H.s.s. ancestor in the same region and both crossed over to the Pacific side at about the same time and each spawned many daughters over many millennia — by inference from these, they are probably each other's closest linguistic kin. Proof we may never get, but who is trying anyway? There are indubitably 100 times as many good linguists tidying up the wee corners of Slavic. Grrr!

3) Somewhere in northeastern or eastern Asia lies the nursery of the native Americans. Theirs is again a case of a fait accompli — their settlement of the New World — creating a need to find their "isolated and separated" place. In the geography of the modern world, Siberia just west of the Bering Straits seems the most reasonable place, at least the closest place, to look. But climate ought to get some consideration. Not only is that Siberian area very far north (65N on average), it is one of the coldest places on earth. Are we required to believe that an emigrant population of Africans plodded across Asia for millennia until they found an icebox of their own to nestle in? Are there no alternatives to this "doing it the hard way?"

There are actually two which make good sense. No doubt others make sense too. The first alternative is given by the general theory presented elsewhere in this issue (pp. 58-61); it favors the maritime route from Japan up through the Kuriles to the Aleutians to the northwest coast of North America to California — then inland. It does not require a crucial period of lowered sea level, nor does it have bottlenecks through glacial gulches in the Rockies. It gets its genetic drift by canoe loads of people, one of the original examples used to explicate the drift concept. But, indeed, it might require some boats. One doubts that Thor Heyerdahl's rafts could make it far in the north Pacific. But island hopping by rafts with some sweepstakes aspects to it might have done the trick.

The second alternative is given by physical anthropology in its discussions over the years *plus* the youthful dates for the Amerind settlement of the New World. Although Douglas Wallace and his team have muddied up the Mongoloid waters somewhat by linking Amerinds to southeast Asia (see MT-17), the long term conclusion of most anthropological studies has been that native Americans are derived from the larger and older population of Mongoloids in Asia of the middle latitudes (from Japan to the Altai) — what Christy Turner calls the Sinodonts. The famous theory that Mongoloid peoples are "cold-adapted" does not require that they live up in

Yakutia to become so; cold windy areas like Manchuria, Mongolia, or northern China will do nicely. Besides that, the vast majority of modern Mongoloids live in the temperate zones of the earth.

The general probability which we discussed before is that the Amerinds entered North America between 20 kya and 40 kya, although the data on Nootka are insufficient for describing the diversity of the hundreds of Amerind peoples, still the mtDNA dates for the Nootka (given by Harpending et al.) suggest that $36,000 \pm 2,000$ might be right. This alternative would thus stress that native Americans are an early expansion from an older Mongoloid nesting place. The likelihood that that older population was partly cold-adapted gives us a basis for believing either in the original staging area in northeast Asia or in the maritime route. (The Aleutians are not really tropical.) Where then is the mongoloid nursery?

4) We will have to continue looking for it somewhere, but the next region *might* be it; that is to say — south China or mainland southeast Asia. The people of the region phenotypically often resemble the Mongoloids of the north. But both Cavalli-Sforza's team, and Wallace's, have strongly indicated a general population with genotypic affinities to India and the far Pacific but otherwise distinctive. Except for their grand conquest of much of the Australoid realm and their vast voyages into the Pacific, the regional population is still living where it began. Unhappily, the tendency of the biogeneticists to lump people together as "Asians" prevents us from saying what differences Harpending et al. found between the northerners and southerners.

But Cavalli-Sforza and his team separate northern Mongoloids from southeast Asians (or "southern Mongoloids") and indeed make those folks more akin to the Australoids. Arthur Steinberg's collation of Gamma Globulin research does essentially the same. Christy Turner's Sinodont vs. Sundadont distinction accomplishes the same splitting up of the northerners and southerners. A generation ago, physical anthropologists often called the southerners "unspecialized mongoloids" and the northerners "specialized Mongoloids." Since the American Indians were also called "unspecialized," i.e., not molded by severely cold climate, that linked them to southeast Asians.

But a better way of seeing these relationships was suggested in 1959 by William Howells in his *Mankind in the Making* (Doubleday, Garden City, NY). On page 300-301, he said, speaking of the skeletons in the Upper Cave at Choukoutien (north China): "Actually, this apparently strange assortment in the Upper Cave really looks like a group of American Indians." (Later) "As to the appearance of the specialized Mongoloids of Siberia, these Upper Cave people seem to say the same thing as do the Indians (i.e., Amerinds — HF). Dating from some time not long before the emergence of Mesolithic culture in the Far East, they are not 'specialized', but only moderately 'Mongoloid'. The Indians, arriving in America over a long period, down to an even later time, are also not specialized. The Eskimos are specialized; they are late, not before about 1000 BC in America. Thus, whenever the specialized Mongoloids became established, they were not general in eastern Asia until late times, as we saw earlier in this chapter. This raises the unspecialized, basic Mongoloids to a

still higher plane of importance; it is hard not to see them as an old Asian population of considerable importance, later overrun and affected by the specialized type. I think one might say the older Mongoloids were probably more like the Whites in early days, and the Whites more like them; that is to say, like the American Indians."

On the next page, he calls his own remarks "rank speculation" but estimates dates of 35,000 to 60,000 BC, presumably referring to the separation and splitting of the "Whites" and the varieties of "Mongoloid." We should all be so lucky with speculations! A final note on southeast Asia must be that it is a densely populated area linguistically. Moreover, its colonies in the far Pacific are nowadays known to be from western Sundaland or south China (from the Yangtse river or Shanghai to Vietnam). All the great phyla of southeast Asia are native, except Sino-Tibetan, which may not be because of its alleged ties to Dene-Caucasic. Southeast Asia has got to be another of the great nesting areas, no matter how the matter of the "Mongoloids" is resolved.

5) It may be very strange for an Africanist to say this, but I cannot see any clear nesting place or staging area for Africa as a whole. Harpending et al. say that Africans started out from their nests circa 90 kya. That is only shortly after the great African Diaspora itself. So essentially what they are proposing is 900 centuries of differentiation in the bodies and in the languages of Africa, not the usual 400 or 500 centuries. But this places a severe burden on linguistic taxonomy, because it seems to be asked to reach into a far remoter past to find taxonomic ties than other areas are asked. What are the solutions to this? All Africanist colleagues are invited to think, then write to us.

Conclusion

There seem to be only three clear regional nesting areas outside of Africa — the inner Near East, eastern Sundaland, and southeast Asia. That Africa as the oldest region may have some sub-regions goes without saying. They might turn out to be simply the homelands of each of the four great African phyla. But the old Asian area which possibly sheltered the "Mongoloids" eludes us. Just who we are talking about when we say "Mongoloids" is still pretty fuzzy. We have some work to do!

FROM THE NEW WORLD:

OLD COLORADO CAVE WOMAN — AND MAN

John Noble Wilford of the *New York Times* reported the highlights of a conference on Rocky Mountain anthropology, held at Jackson, Wyoming, presumably late September, 1993. The *Times* article was dated October 3, 1993, p. 13, and it was entitled "8,000-Year-Old Human Bones are Found in Cave." The salient paper at the conference was given

by Patty Jo Watson (Washington University, St. Louis) and concerned a robust human male who had apparently frozen to death in a high altitude cave (10,000 feet or 3030 meters) in what is now the eastern edges of White River National Forest. There were almost no artifacts with the man, save a pine torch which left smudge marks on the cave walls and charcoal fragments on the clay floor. Wilford said geologists had concluded that the human bones had not intruded into the cave, had not been "eroded in" — such bones as his skull cap, right upper arm, right and left thighs, some ribs and vertebrae, right and left lower legs, fragments of the pelvis, four finger bones and 11 teeth. Clothing was thought to have been animal hides and probably eaten by small animals. A good forensic anthropologist could probably tell us what his wife's name was too.

In addition to the bones and teeth (attention Christy Turner!), "Scientists have just begun analyzing DNA material extracted from the bones. Early results show genetic patterns known to be present in American Indian populations in regions south of Canada". The man was possibly an ancestor of the Southern Utes, present occupants, to whom the bones were given. "Casts, x-rays and photographs are available to scientists for further research." (The United States Forest Service is custodian.).

One is permitted a speculation that the man was more likely to be an ancestor of a large family like Uto-Aztecan, rather than a single tribe like the Utes or Paiutes, since eight millennia in the past those tribes had not yet appeared as individual entities — probably.

However, old Colorado man was not the oldest human found in North America, although he was the oldest high altitude fossil found anywhere on earth; the Ice Man of the Alps was found at 10,500 feet (3,182 meters), but he was only half as old as Colorado man. Egyptian mummies of the most ancient pyramid building period were essentially contemporaries of the Ice Man.

The oldest fossil person found in North America was a *woman* of 9,000 years ago, also found in Colorado but not at such a high altitude. (Most of Colorado, however, qualifies as "highlands"). Right now, we have no more information on her. Although it is not likely, we might be able to read some mtDNA from her bones. That would help a lot because, if her haplotype is such, it may locate one of the early mtDNA lineages, and it might help to date the common mtDNA ancestry (in Berber it would be */*t-ancestor-t*) of Amerinds. As Paul Benedict says: "seek the earliest possible forms."

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AFTER THE CLASSIC MAYAN COLLAPSE, ONE CITY SURVIVED FOR A WHILE

While we have not been reporting the broadening wealth of the recent discoveries about the civilized Maya of Mexico, Belize, Honduras, and Guatemala, there is much news from that topic. But, of course, we have not been reporting

much on ancient Egypt, Sumeria, or Anatolia either, unless the discoveries were grist to our mill. It should be mentioned that quite a few new books and articles have become available on the writing system of the Maya and its interpretation. The excitement in the air reminds one of the grip on our imaginations that Egyptology once had.

Indeed the Maya writing system, which is parent to all New World indigenous writing systems, except Cherokee, strikes one as much more akin to ancient Egyptian writing than to any other. While the Indus Valley script is also closer to Mayan, the basic cuneiform of Mesopotamia is strikingly different. In a nutshell, the Mayan writing most resembles a rebus system set in sculpture or pictures. This is not to imply in any way that the Mayan system owed anything to the Egyptian or Indus or Chinese systems which are most like it.

Many old hypotheses about the Mayan civilization have had to be abandoned recently as archeology has exploded with information about it. It is now clear that it was an urban civilization, but not a peaceful one; city-states with walls and 50,000 or more inhabitants, with monumental architecture, especially pyramids, mathematics much superior to that of the Greeks or Romans, accurate calendars, efficient farming, widespread trade, and a religion vastly different and interesting. Like the Roman empire or Greco-Roman civilization, its had a more or less dated ending — around the 9th century A.D.

And like Rome, it did not fall all together at the same time. The city of Xunantunich [šunántonitš] lasted another century or two in Belize. It is thought to have been peaceful, lacking city walls, and may have survived because it was *not warlike*.

The new report on this city is based on its ceramics primarily. The principal investigator was Richard M. Leventhal, director of the Institute of Archeology at U.C.L.A. The article on the city was written by science reporter John Noble Wilford in the *New York Times*, pp. B5-6, October 5, 1993. For those who like vivid pictures and reconstructions, besides a good text, there have been large articles in the *National Geographic Magazine* and the *Sunday New York Times* and books by Michael Coe and others. We are not going to follow that literature here, as it is excessive for us.

One interesting thing about pyramids. They produce for archeology the problem of "look-alikes," so maligned by Paul Benedict and most linguists. Why? Pyramids in Egypt and Mexico have provoked many attempts to show how the Mexican pyramids were derived from a chain of migrations or simpler diffusion from Egypt. Yet it is only their "gestalt's" which are similar. Each type was built fundamentally differently and had a grossly different purpose than the other. Actually, the temple mounds or ziggurats of Mesopotamia were more akin to the Mayan pyramids than the beautiful Egyptian tombs (pyramids) were. Bien touché, Paul!

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MORE ON mtDNA OF AMERINDS AND SIBERIANS AND SOME PHYLETIC DATES

Two significant articles have appeared in the *American Journal of Human Genetics* this year. They were back-to-back in volume 53 but had different authors and titles.

Thanks to our roving correspondent, Alvah Hicks (Ojai, CA), we will have a more intensive analysis of both articles in the next issue, written from another perspective. Hereinafter, we will focus on the key points from the global discussion perspective.

The first occupied pages 563-590 and was written by Antonio Torroni, Theodore G. Schurr, Margaret F. Cabell, Michael D. Brown, James V. Neel, Merethe Larsen, David G. Smith, Carlos M. Vullo, and Douglas C. Wallace. All but Neel, Larsen, Smith, and Vullo are at Emory University (Atlanta, GA); Neel is at the University of Michigan, Smith at the University of California at Davis, and Vullo at the Universidad Nacional de Cordoba, Cordoba, Argentina. Larsen may be reached at Department of Medical Genetics, Tromsø, Norway. For correspondence and reprints, write Wallace, Department of Genetics and Molecular Medicine, Emory University, Atlanta, GA 30322.

The first paper's title is "Asian Affinities and Continental Radiation of the Four Founding Native American mtDNAs." Its "Summary" says: "The mtDNA variation of 321 individuals from 17 Native American populations was examined by high-resolution restriction endonuclease analysis. All mtDNAs were amplified from a variety of sources by using PCR. The mtDNA of a subset of 38 of these individuals was also analyzed by D-loop sequencing. The resulting data were combined with previous mtDNA data from five other Native American tribes, as well as with data from a variety of Asian populations, and were used to deduce phylogenetic relationships between mtDNAs and to estimate sequence divergences. This analysis revealed the presence of four haplotype groups (haplogroups A, B, C, and D) in the Amerind, but only one haplogroup (A) in the Na-Dene, and confirmed the independent origins of the Amerinds and the Na-Dene. Further, each haplogroup appeared to have been founded by a single mtDNA haplotype, a result which is consistent with a hypothesized founder effect. Most of the variation within haplogroups was tribal specific, that is, it occurred as tribal private polymorphisms. These observations suggest that the process of tribalization began early in the history of the Amerinds, with relatively little intertribal genetic exchange occurring subsequently. The sequencing of 341 nucleotides in the mtDNA-loop revealed that the D-loop sequence variation correlated strongly with the four haplogroups defined by restriction analysis, and it indicated that the D-loop variation, like the haplotype variation, arose predominantly after the migration of the ancestral Amerinds across the Bering land bridge."

A quick observation would be that nothing in their data or analysis indicated that the Bering land bridge was involved. They simply made that assumption. Fascinating,

what! Do preconceptions get filtered out by big labs, lots of data, advanced computers and powerful statistics? Apparently not.

In the first paper, Torroni et al. decided to follow the Greenberg classification, instead of its alternative with scores of independent phyla (see Campbell and Mithun, 1979). Naturally, it is better to compare a unifying biogenetic theory with a unifying linguistic one. However, the surprise is that their biogenetic results need not be fitted to a unitary linguistic one. (More later) The "tribes" used in the study, their sub-phyla and locations are as follows:

Northern Amerind: Bella Coola, Nootka, Ojibwa, and Maya. All are Almosan-Keresiouan, except the Maya (Penutian). All live in northwestern USA or Canada, except the Maya (Meso-America).

Central Amerind: Pima of Uto-Aztecan. They live in Arizona.

Chibcha-Paezan: Boruca, Kuna, Guaymi, Bribri/Cabecar and Yanomama. All are of the Chibchan branch; all live in Central America, save the Yanomama, who live in northern Amazonia. The Kuna are normally called the Cuna.

Equatorial-Tucanoan: Piaroa, Wapishana, and Ticuna (who are mistakenly listed as the Almosan-Keresiouan branch of the sub-phylum! They should be listed as Ticuna-Yuri.) The first two belong to the Macro-Arawakan branch of Equatorial, while the Ticuna belong to the Macro-Tucanoan branch of Tucanoan. All live in the eastern Amazon, all in Brazil.

Ge-Pano-Cariib: Makiritare, Macushi, Kraho, Marubo, and Mataco. Makiritare and Macushi belong to the Macro-Carib branch of the sub-phylum, Kraho to Macro-Ge, and Marubo and Mataco to the Macro-Panoan branch. Another Amazonian branch and more to the south of Equatorial-Tucanoan than not. However, the Makiritare live near the Yanomama and are known to have exchanged genes with them. This shows up in the results.

Andean: This important sub-phylum is not represented; that is unfortunate because such high altitude adaptations as the Aymara or Quechua, or such adaptations to frigid maritime conditions as the Ona or Yahgan or Alacaluf, are left out. Or the most important simple fact that they are much farther south than other Amerind groups and thus are arguably representative of the oldest population of the New World. However, it is foolish to criticize a study which has gone far beyond others just because it was not perfect. (Of course the Lakota or the Iroquois would have been nice to include, too!)

Na-Dene: The Dogrin, Navaho, Apache, and Haida. The first three represent "Continental" Na-Dene, which many would simply call Athapaskan or Eyak-Athabaskan, while

Haida represents itself. Tlingit or one of the California Athapascans would have been nice too, but the diversity in Na-Dene is quite well represented.

Perhaps the most arresting finds are the shared innovations (shared mutations) of all the Chibchan (minus Yanomama) in one set and most of the Ge-Pano-Carib plus Yanomama in another. Without the technical details (p. 584), one mutation ties together a linguistic group which is quite distinct in Central America, what Greenberg called the "Nuclear Chibchan" group of Chibchan. The other shared mutation links members of Ge-Pano-Carib (GPC) with each other but not some others of their sub-phylum. The Chibchan Yanomama share the haplotype mutation with these GPC groups, while the kindred GPC Makiritari do not share it. Yet they probably "loaned" it to the Yanomama at a time in the past when they possessed it. This is equivalent to a linguistic group having an ancestral and defining innovation which is lost to some of the descendants but loaned to an outsider at an earlier date who still keeps it. (Examples probably can be found in the case of proto-Germanic vis-à-vis Finnic.)

There are plenty of results to indicate that Haida belongs with the other Na-Dene but lacks a special mutation common to the others, consistent with its position as an isolate or coordinate within Na-Dene. And results that affirm that Na-Dene is a biogenetic entity separate from the other "native Americans".

Other results show that the Amerinds generally are a coherent whole, that they derive from general East Asia rather than the far northeast of Siberia (more like Wm. Howell's concept of the basic Mongoloids), that there were four founding "lineages" among them, and that Na-Dene was *not* one of them. On page 584, they say: "In summary, the distribution and frequency of mtDNA haplotypes in the Americas, and their phylogenetic relationships to Asian (sic) and Siberian mtDNAs, appear to indicate that haplotypes *AM1*, *AM13*, *AM43*, and *AM88* were the founding haplotypes for all modern Amerind mtDNAs." (My emphasis — HF) These haplotypes belong to haplogroups A, B, C, and D respectively. The Na-Dene conversely entirely lack haplogroups B, C, and D except that the Navaho have acquired some B because of taking Pueblo wives.

Since haplogroup B is divergent within Amerind, one might think it possible that it represented a separate migration from the others and that this migration represented the arrival of the Na-Dene in the New World. Yet it does not. Na-Dene in general correlates heavily with haplogroup A — only A. But Torroni et al. do think it possible that a separate migration brought haplogroup B to the Americas, perhaps via coastal Siberia. (They must have been reading the latest *Mammoth Trumpet*.)

Finally, Torroni et al. state their most important findings: "To estimate the entry time of the first Americans into the New World, we calculated the sequence divergence values for each of the four mtDNA haplogroups (table 8). These calculations were based on both the sequence variation estimated from the restriction site haplotypes of all Amerind tribal groups and the consensus mtDNA sequence evolution rate of 2.0%-4.0%/MYR . . ."

"The sequence divergence that has accumulated since

the four Amerind haplogroups began to diverge from those of Asians and Siberians is 0.091% for group A, 0.024% for group B, 0.096% for group C, and 0.053% for group D. The average divergence value for all four haplogroups is 0.067% (table 8), which gives an overall divergence time of 16,750-33,500 years before present (YBP). As the Clovis culture is assumed to have begun in America about 13,500 YBP, the mtDNA data appear to support a pre-Clovis colonization of the New World."

"Haplogroup B, the haplotype group associated with the 9-bp deletion, showed the lowest sequence divergence and, consequently, the lowest divergence time of 6,000-12,000 YBP." Difference in divergence time between haplogroups could simply reflect the large intrinsic error involved in these estimations. However, an extensive mtDNA analysis of aboriginal Siberian mtDNAs (Torroni et al. 1993. The 2nd paper — HF) failed to reveal any group B mtDNAs, even though groups A, C, and D were widely dispersed in Siberia. This raises the possibility that group B mtDNAs came to the Americas through a different route and, therefore, possibly at a different time than that of the other haplogroups. If haplogroup B is removed from estimating the overall sequence divergence for Amerind mtDNAs, the average sequence divergence for the remaining haplogroups is 0.082% (table 8), and the time of divergence becomes 20,500-41,000 YBP. In either case, the mtDNA data provide divergence times which are consistent with a pre-Clovis origin of the first Americans."

To put the matter in simpler terms, proto-Amerind is likely to be a genetic unity and to have arrived maybe as recently as 17,000 years ago or as long ago as 41,000 years. But the middle ground in these dates is circa 25,000 years ago \pm 8,000 years or, without haplogroup B, then 31,000 years ago \pm 10,000 years.

Probably by accident, Torroni et al. forgot the Na-Dene dates, which can be taken from their recent work (reported in MT). They were 5,250-10,000 YBP or on the middle ground 7,900 YBP \pm 2,600 years. One may venture to say that a proto-Na-Dene began to break up and move away from its homeland between Juneau (Alaska) and Vancouver some time after the 6th millennium BC, judging from the cluster of basic branches (Haida, Tlingit, Eyak) still on the coast. Just as the Navaho partake of Pueblo genes, the Haida have a considerable amount of "Caucasoid" in their pool. The Russians!

What we are to do about the *three* founding lineages of the Amerinds depends a great deal on how those are defined and how the biogeneticists explain their meanings. One possibility can be tossed out, just because it can be imagined. Perhaps Amerind broke up soon after it arrived and spread out rapidly to occupy the two virgin continents. Under that assumption, perhaps the three founding lineages can be identified with the three huge sub-phyla which Ruhlén proposed earlier, to wit, Northern, Central, and Southern. (When Amerind is recognized as the super-phylum it really is, then such groups as Central Amerind will be called phyla or even macro-phyla.) Another assumption might be that the original Amerinds had come from a population of generally similar people from a fairly large area in Asia but that they did not speak the same languages. So, pouring into the land of opportunity in Alaska and/or the Yukon, they segregated

themselves as to routes, thus creating the appearance of bottlenecks. Possible?

The *second paper* was co-authored by Antonio Torroni, Rem I. Sukernik, Theodore G. Schurr, Yelena B. Starikovskaya, Margaret F. Cabell, Michael H. Crawford, Anthony G. Comuzzie, and Douglas C. Wallace. Sukernik and Starikovskaya are at the Section of Human Molecular Genetics, Institute of Cytology and Genetics, Russian Academy of Sciences, Siberian Branch, Novosibirsk; Crawford and Comuzzie are in the Department of Anthropology, Laboratory of Biological Anthropology, University of Kansas, Lawrence (Kansas). The rest are at Emory University. The paper is entitled: "mtDNA Variation of Aboriginal Siberians Reveals Distinct Genetic Affinities with Native Americans." It occupied pages 591-608 of *AJHG*, following right after the first paper. Its Summary said:

"The mtDNA variation of 411 individuals from 10 aboriginal Siberian populations was analyzed in an effort to delineate the relationships between Siberian and Native American populations. All mtDNAs were characterized by PCR amplification and restriction analysis, and a subset of them was characterized by control region sequencing. The resulting data were then compiled with previous mtDNA data from Native Americans and Asians (sic) and were used for phylogenetic analyses and sequence divergence estimations. Aboriginal Siberian populations exhibited mtDNAs from three (A, C, and D) of the four haplogroups observed in Native Americans. However, none of the Siberians showed mtDNAs from the fourth haplogroup, group B. The presence of group B deletion haplotypes in East Asian and Native Americans but their absence in Siberians raises the possibility that haplogroup B could represent a migratory event distinct from the one(s) which brought group A, C, and D mtDNAs to the Americas. Our findings support the hypothesis that the first humans to move from Siberia to the Americas carried with them a limited number of founding mtDNAs and that the initial migration occurred between 17,000-34,000 years before present." End of quoting.

Two quick observations: First observation: since research and scientific effort are oriented or focused on testing hypotheses or in solving problems by proposing hypotheses, this is a very focused effort. They wanted to see the relationships between the native populations in the geographical area most likely to be *the first ultimate homeland* of Amerinds, or call it the *pre-Proto-Amerind* homeland, and the native populations of the Americas, and they wanted to *date* both the separation between the Siberians and Amerinds and the splitting up of the early Amerind population. In this, they appear to have succeeded admirably — they stipulate a relationship and they date *two dispersions*, the one of the Siberians and the other of the Amerinds.

Second observation: they assume that the native Siberians represent the kind of people the Amerinds are most closely related to and they assume that the native Siberians are a valid category. The latter seems to mean non-Europeans and non-"Asians" who live in the Russian empire east of the Urals. Amerind does not quite match up because it is geographical (the Americas), it is non-European and non-African, but it is also linguistic, i.e., it is not Na-Dene or Eskimoan but explicitly

Greenberg's Amerind (in the companion paper). Hence, they missed a great chance to test the entire Eurasiatic / Eastern Nostratic hypothesis by including the Japanese or Koreans, the Ainu and the Lapps or Finns and some IE speakers, like Russians. For all of those, some data were already available in their so-called Asian group.

Nevertheless they did test and *date* the Eurasiatic superphylum, as we will see at the end of this.

Perhaps another observation is pertinent. Because so much of the recent thrust of biogenetic papers has been on the eastern half of the Old World, especially China and regions north, south, and east of it, the concept of *Mongoloid* is overdue for closer scrutiny. By bringing in some of their own data on "Australoids" and Europeans, they could test the Mongoloid theory and perhaps assign a date to the formation and/or dispersal of this "race." It is particularly pertinent because Cavalli-Sforza, Piazza, et al. are on record as stating that non-mtDNA genetic analysis shows that northern "Mongoloids" are most closely related to Europeans — not southern "Mongoloids."

Nevertheless, it is still foolish to criticize a good paper on the grounds that it is not perfect. And this is a great paper!

Who were the Siberians tested for mtDNA? Extending from the Bering Straits almost to the Ural mountains, they embrace *most* of the smaller phyla incorporated into Eastern Nostratic or Greenberg's Eurasiatic. *Uralic* is represented by two Samoyed groups, Selkup and Nganasan, and the Yukagir (Yukaghir) by themselves. *Altaic* is represented by the Tungusic speaking Evens (Lamuts), the Evenks, and the Udegeys (south of the Amur River basin). *Chukotan* or *Chukchi-Kamchatkan* is represented by the Chukchi themselves and the Koryak. The Asiatic Eskimo or Yupik stand for the *Eskimo-Aleut* phylum, while the Nivkhs (*Gilyak*) stand for themselves.

What is odd about the assortment linguistically is that nowhere was it suggested that a linguistic taxon had been created that binds these languages together! For the eastern half of Siberia, they used the conservative views of Michael Krauss, who did not think the Yukaghir or Gilyak were related to anyone else, although a Russian (Kreynovich, 1978) thought Yukaghir "could be related to the Samoyedic branch of the Uralic linguistic . . ." Not only was the Greenberg's Eurasiatic ignored by the same people who used his Amerind hypothesis in the Americas, but even the famous Nostratic of Illič-Svityč was ignored by the Russians. Even two who had supposedly read *Mother Tongue* for several years, could not remember anything said about Eurasia. There must be a lesson in this somewhere, eh? What is it?

But now for the good news! Torroni et al. found that the Siberians hung together as a biogenetic cluster and that they as a cluster were younger than the Amerinds as a cluster. Two aboriginal populations — Siberians and Amerinds — had budded off from the "Asians" and had gone their separate ways. Because not all the data from some of the Siberian populations were adequate, hence could not be included in final calculations, the age of the Siberians might have been *underestimated* somewhat. Since so many of the haplotypes were so different on both sides of the Pacific, each group must

have been separated from the other for most of their histories. Just as a corollary of that, the Amerind diversity arose independently in the Americas. In fact (page 604), "... little, if any, of the mtDNA diversity that currently exists in Native Americans arose in Siberia prior to the Amerind migration. Therefore, the genetic diversity that exists on each continent can be considered proportional to the time that these populations have been separated."

What was that time? The same 17k to 34k we heard about in the first paper. But what about the Siberians? Their divergence is 15-30k for haplogroup C and 10-20k for haplogroup D. Combining C and D gives a slightly different sequence divergence (0.054), yielding a final time estimate of *between 13,500 and 27,000 YBP* for the Siberians. The middle ground is 20,250± 6750 years.

Remember the haplogroup B which did not fit properly in amongst the Amerinds and which was absent among the Na-Dene? Well, the Siberians have none of it. Where is it found? Among the "Asians" naturally.

We have reported enough on the second paper at this point. Do read it on your own. Or wait for Alvah Hicks' article on it in a later issue of *Mother Tongue*. For the nonce, we have other fish to fry. Torroni et al. have done a remarkable job of giving some reasonable dates, biogenetic dates tis true, which can probably be associated with no less than three linguistic taxa. They will show in all likelihood that our phyla and super-phyla *do differ in age a lot*. When one considers how much linguistic diversity associates with time, and how much diversity associates with difficulty of classification, then the older the phylum, the more difficult it is to get people to accept it — or even to propose it. Consider that Na-Dene is mildly controversial, but only because of Haida. Now see how Nostratic is controversial but steadily gains adherents. Finally, look at Amerind, which is a fight to the death between proponents and opponents. Now look at these likely dates:

proto-Na-Dene	7,900 years ago ± 7.6k
proto-Eurasiatic	20,000 years ago ± 7k
proto-Amerind	25,000 years ago ± 8k or 31,000 years ago ± 10k

Without much doubt, Indo-Pacific is older than any of these, while Australian is likely to be at least as old as Amerind. Yet look at the time frame the traditional linguists are stuck in:

proto-Indo-European	5,500 years ago ± 1k
proto-Uralic (sans Yukaghir)	6,000-8,000 years ago (Anttila 1989, p. 301)

Since the traditionalists cannot seem to break out of the mind sets given them by their shallow phyla, good advice to them would be to try working for a while in something a bit deeper.

NOTES

1) The linguistic analogy to pyramids on closer examination comes to this. In relation to Mayan pyramids, those of Egypt can be said to have the *same form but different meaning*. In relation to the Mesopotamian "ziggurat," the

Mayan pyramids had *different form but same meaning*. Finally, the Egyptian pyramid and Mesopotamian "ziggurats" have different form and different meaning. So by the way linguistic minds work, none of these structures are *cognate*, just "look-alikes." Methinks they're right!

2) The reference to (Kreynovich, 1978) above is to F. A. Kreynovich, 1978. "Of some Yukagir-Uralic Parallels," in: *Sovetskoye finno ugrovedeniye*, vol. 14. Academy of Sciences of the Estonian SSR, Tallinn, pp. 214-249. (In Russian — not read by Hal.) Another recommended source would be M. G. Levin and Potapov, eds., 1964. *The Peoples of Siberia*. Chicago, University of Chicago Press.

3) Sometimes, when one contemplates the resistance of some linguists to accepting Haida as a coordinate half of Na-Dene, the question arises: Have they seen H-J. Pinnow's evidence/arguments for the membership of Haida in Na-Dene? Pinnow's evidence is quite powerful. The suspicion is that Pinnow's papers have not been widely disseminated. If you are interested, write to Uwe Johannsen, Volkerkundliche Arbeitsgemeinschaft, Postfach 1142, 2353 Nortorf, Germany. (Number 2353 is the old zip code. Germany has new ones, but the postmen will know where Nortorf is.) Uwe should be able to get you copies.

4) So *that* is why they don't accept Pendejo Cave? A very thoughtful little piece appeared in *Science*, Vol. 257, 31 July 1992, pp. 621-622. Entitled "The Perils of a Deeply Held Point of View," it was written by Eliot Marshall. On the subject of Scotty MacNeish, Pendejo Cave, and Scotty's persistent search for proof of pre-Clovis habitation in the New World, Marshall interviewed a number of archeologists, some of whom were fellow "iconoclasts" and some not. Collectively, they disapproved of Scotty's style, his habit of going to the popular press, his jumping to conclusions, but most of all his persistent quest in site after site for pre-Clovis dates and cultures. Since so many of his earlier sites were judged to be duds with respect to his quest, that was held against him too. Translated roughly into common speech, it means: this guy is a fanatic and a loser, so do not believe his results. Yes, he is a great archeologist, a great field archeologist, the discoverer of the Mexican Neolithic, and a prestigious person. But still . . .

Two archeologists who were long rangers had said as much to me previously. Or as one put it: "He keeps finding these sites; that's what bothers me." To put it another way, Scotty MacNeish has lost his credibility — he cried "Eureka!" too many times.

Everyone knows that I have supported MacNeish's Pendejo Cave site and his conclusions — in *Mother Tongue* and several times. Sue DiCara of El Paso and I looked the site over carefully and thought it looked very competently dug. Of course, we are not archeologists, but so what? Very few of the opinions expressed by MacNeish's judges had much to say about the excavations. They homed in on the "poor quality" of the artifacts at the lower levels. Those opinions were not based on their own observations; they were based on the opinions of a couple of Hrdlička type "experts" who had seen the site. So it was "hear say" evidence.

It took archeologists collectively a few years to admit that their famous skepticism was actually personal. That and

believing negative opinions by "experts" because it was easier. How many years do you think it will take before archeology makes an honest evaluation of Pendejo Cave by evidence, not rumor?

While the bulldog is associated with the English, dogged persistence is often characteristic of Scots — and creative scientists — and fanatics — and great detectives. So surely doggedness per se does not make us wrong; maybe it is adaptive to hang on to this bone.

NEWS FROM THE "HARDWARE" FRONT

For ASLIPers who are also LOSers, there is no need to review the research and argumentation evolving around the question of *when* and *how* and *in whom* or *among whom* human language evolved. We have rather shamefully neglected most of those questions for some time now. This is in fact partly due to a somewhat testy relationship between the two camps and partly due to basic differences in organization. The Language Origins Society (LOS) is largely an annual conference (plus book) organization; ASLIP is basically a newsletter (plus journal features) organization.

As we have argued in the past, we two groups concentrate on basically different problems to be labeled the "hardware" and "software" aspects of language. The "hardware" is conceived of as the evolutionarily derived and ontogenically acquired basic human capacity to *speak* one of the 5000+ human languages. No other critters in the universe, we all repeat like a mantra, can speak human language. Some of the LOSers, however, do not see speech as the core of language; we have different definitions of language.

It is time for some discussion of the "hardware" again. Not only have there been several long papers and discussions in *Current Anthropology* this year on the subject of language origins, but also one of our founding long rangers, Phil Lieberman of Brown University, has been deeply involved in much of it. We have not had time to review, e.g., Robbins Burling's article in *Current Anthropology*, and we may never do it, but we have the benefit of abstracts from four of Lieberman's recent papers to help us out. Of course, this is presenting only one side of several debates, but we hope that ASLIPers will be stimulated enough to examine other views. For the professional and more grammatically oriented linguists, the issues should be seen as *hot* because the underlying structure (heh, heh) of Chomskyite theory is being clobbered. (Much of that should appear in a later issue.)

One thing to announce is that a *paperback* edition of Phil's challenging book *Uniquely Human: The Evolution of Speech, Thought, and Selfless Behavior* (Harvard University Press) has now come out. Much cheaper than the original hard cover. One day he will also give us his research results from a recent field trip to Nepal, studying the effects of altitude on speech.

The four key issues discussed in his papers are

Neanderthal speech (especially the hyoid bone), the "autonomous syntax module," the anatomy of human speech, and the testimony of some neurological diseases on these matters. Let's do the papers directly.

Appearing in the *Journal of Human Evolution* (1992) 23, pp. 447-467, and authored by Philip Lieberman, Jeffrey T. Laitman, Joy S. Reidenberg, and Patrick J. Gannon, the article is entitled: "The anatomy, physiology, acoustics and perception of speech: essential elements in analysis of the evolution of human speech." The abstract is tough going for scholars outside of the technical specialties involved but is worth studying slowly. It says:

"Inferences on the evolution of human speech based on anatomical data must take into account its physiology, acoustics and perception. Human speech is generated by the supralaryngeal vocal tract (SVT) acting as an acoustic filter on noise sources generated by turbulent airflow and quasi-periodic phonation generated by the activity of the larynx. The formant frequencies, which are major determinants of phonetic quality, are the frequencies at which relative energy maxima will pass through the SVT filter. Neither the articulatory gestures of the tongue nor their acoustic consequences can be fractionated into oral and pharyngeal cavity components. Moreover, the acoustic cues that specify individual consonants and vowels are 'encoded', i.e., melded together. Formant frequency encoding makes human speech a vehicle for rapid vocal communication. Non-human primates lack the anatomy that enables modern humans to produce sounds that enhance this process, as well as the neural mechanisms necessary for the voluntary control of speech articulation. The specific claims of Duchin (1990) are discussed."

The last reference is to L. E. Duchin (1990), "The evolution of articulate speech: comparative anatomy of the oral cavity in Pan and Homo." *Journal of Human Evolution* 19, pp. 487-497. Duchin allegedly misunderstood the "basic facts" of human speech.

Lieberman et al. have a very rich statement of the elements of human speech; too rich to be reviewed entirely here. Read it!

Their conclusions can be given (minus bibliography): "Ultimately we all would like to determine as much as we can concerning the biology and behavior of fossil hominids. The answers to some questions will probably always remain uncertain. Since we have no specimens of the soft tissue of the SVT of any fossil, scholars probably will continue to debate the form of the reconstructed SVT of particular fossils. However, the acoustic consequences of particular SVT morphology are not subject to debate. The SVT can be modeled using existing well attested methods. In this regard, the non-human SVT cannot produce speech sounds that facilitate the process of speech encoding which contributes to biological fitness. Therefore, fossil hominids who had SVT unlike those of modern humans were not as well adapted for this species-specific attribute of *H. sapiens*."

"As we noted earlier, human speech also depends on neural mechanisms that again appear to be species-specific. It appears possible to make some inferences concerning the

presence of these neural mechanisms in certain fossils based on SVT reconstruction. As Charles Darwin...noted, the human SVT is maladapted for swallowing and respiration compared to the non-human SVT... Therefore, it is significant to find skeletal indicators of a modern SVT in early members of *H. sapiens* such as Kabwe (Broken Hill) or Skhul V... The SVTs of these early specimens of *H. sapiens* would have reduced fitness, unless the neural mechanisms that regulate the voluntary articulatory maneuvers necessary for human speech were present. Therefore, these reconstructed modern SVTs may be regarded as an index for the presence of the neural mechanisms that regulate the voluntary articulatory maneuvers necessary for the production of human speech..."

"Recent neurophysiological data indicate that these neural mechanisms also appear to be implicated in our ability to acquire and use syntax, and in certain aspects of cognition — particularly in the ability to derive abstract concepts... Therefore, it is possible that the anatomy specific to human speech may serve as a marker for some of the brain mechanisms that characterize human linguistic and cognitive ability. Further studies obviously are in order.

"It is important to note that we cannot state with certainty that these neural mechanisms were absent in fossil hominids who did not have modern SVTs. Speech encoding could have been present, albeit with higher error rates than modern speech, in hominids such as the classic Neanderthals who probably did not have modern SVTs. Archeological data, furthermore, indicate that Neanderthals likely employed some form of language (e.g., Marshack, 1989), a point which has long been noted in discussions of their linguistic capabilities... The issue in question is simply whether their speech (and possibly syntax and thinking) was as efficient as that of modern humans."

One reference specially to be mentioned is Alexander Marshack (1989), "Evolution of the human capacity: the symbolic evidence". *Yearbook of Physical Anthropology* 32, pp. 1-34. We should have mentioned it before — and I should have read it!

Lieberman's second paper was in *Current Anthropology* 33, Number 4, August-October, 1992, pp. 409-410, entitled "On Neanderthal Speech and Neanderthal Extinction". While the article is preoccupied with Lieberman's dispute with B. Arensburg et al. on the significance of the hyoid bone in the human SVT and evolution, it begins with three statements which Lieberman did not challenge; they ought to be challenged. They are not necessarily untrue but rather are too important as statements to be accepted so easily.

C. Graves claimed that "the period from the first contact to total disappearance of Eurasian Neanderthals spanned perhaps 50,000 years". That is to say, contact with H.s.s. Graves wrote "New models and metaphors for the Neanderthal debate" in *Current Anthropology* 32, pp. 513-541.

According to Lieberman, E. Zubrow said that a demographic model of slow, gradual Neanderthal extinction seems most probable in the light of present archeological data and theories concerning Neanderthal and early human social organization, hunting, etc. Zubrow wrote "The demographic modeling and Neanderthal extinction," in *The Human*

Revolution: Behavioral and Biological Perspectives on the Origin of Modern Humans, vol. I. P. Mellars and C. B. Stringer (eds.), pp. 212-231. Edinburgh: Edinburgh University Press.

G. Barbujani and R. Sokal wrote "Zones of sharp genetic change in Europe are also linguistic boundaries." 1990. *Proceedings of the National Academy of Sciences, U.S.A.* 187, pp. 1816-1819. Among other things, they "identified 33 geographical zones in Europe demarcated by abrupt changes in 63 human allele frequencies derived from blood typing and found 31 of them to be coincident with linguistic boundaries. They concluded that language affiliation of European populations plays a major role in maintaining and probably causing genetic differences."

Drawing on these three, Lieberman decided that "Neanderthal speech was the genetic isolating mechanism that ultimately resulted in their extinction." From this, he proceeds to show how the hyoid bone and its role in evolution had been misconceived by Arensburg and his associated authors and finally concludes that "A number of other factors also argue against the claim that Neanderthal speech was the same as ours..."

I tend to believe that Lieberman is right in his contentions about Neanderthal speech (or should we call it quasi-speech?). But there is a doubt whether he needs to associate with the three semi-conjectural hypotheses presented above. For example, Graves depends entirely on what is called Neanderthal and where it is found and what its dates are. It might have taken 50,000 years if one counted the very earliest H.s.s. in some places and the very latest Neanderthals in others. But in an area like western Europe, such reasoning could put the last Neanderthals at 10,000 AD!

Model shmodel — Zubrow's statement is a hypothesis with reasons for believing it. Opposing hypotheses are known, but we do not know the reasons for supposing Zubrow to be correct instead of the opponents.

Barbujani and (Robert) Sokal's hypothesis is a very important one. It needs to be tested *cross-culturally*, not just in Europe, because the claims that are made for it are in fact *global*. This would be a good place for sociolinguistics and dialect geography to confront genetic maps. Since dialect boundaries can often be linked to social, political, and cultural events or factors, one might suppose they would be involved in the same triangle with the gene clusters.

Lieberman's third interesting paper, called "Could an Autonomous Syntax Module Have Evolved?", appeared in *Brain and Language* 43, pp. 768-774 (1992). The abstract reads as follows:

"Darwinian evolution necessitates a contribution to reproductive fitness. Recent studies of aphasia and Parkinson's disease show that functional syntactic ability involves neural structures that also are involved in speech motor control and nonlinguistic cognition. The evolution or presence of an autonomous syntax module is, therefore, implausible."

It will help one to understand the magnitude of this small paper, if we cite the introductory background paragraphs. Quote:

"Many linguists accept 'modular' theories of mind which claim that human syntactic ability derives from an 'encapsulated' module, i.e., a mechanism that is functionally and morphologically distinct from modules governing other aspects of human behavior. (Fodor, 1983; Chomsky, 1986). It seems clear that no living species other than *Homo sapiens* has the ability to communicate using complex syntax. Although closely related nonhuman primates such as *Pan troglodytes*...and *Pan paniscus*...can acquire lexicons and communicate at about the level of 2- to 2½ year-old human children using manual-visual output (American Sign Language or pictographic keyboards), neither the sudden expansion in the lexicon nor the sudden transition to complex sentences characteristic of human children occur. Therefore, the module that underlies human syntactic abilities must be regarded as a unique species-specific attribute of *H. sapiens*. Nevertheless, we must account for its evolution. This raises certain problems when we consider the nature of Darwinian evolution and recent studies concerning the neural bases of a functional syntax module."

"Two rather different evolutionary scenarios have been proposed. modular solutions have been proposed by Bickerton (1990) and by Pinker and Bloom (1990). Bickerton (1990), working in the Chomskyian (1972, 1986) tradition, claims that no homologue to the hypothetical syntax module exists in any other species. According to Bickerton, an autonomous syntax module, evolved by means of a single, sudden genetic change that dramatically modified the human neocortex, creating cortical maps, encoding grammatical relations and a 'syntactic' wiring diagram. Although this solution limits itself to the neocortex, it is implausible; a vast number of coordinated changes would have had to occur at once for the full system to function. Since an autonomous syntax module has no function other than syntax, it must be operable. As Chomsky has argued (1980), there would be no selective value for a partially specified inoperable syntax module. Therefore, all the necessary neural hardware must simultaneously fall into place."

"Pinker and Bloom (1990) propose another solution that they believe would yield an autonomous syntax module. They differ with Chomsky...and adopt a Darwinian model of evolution. The details of their solution are vague, but they essentially propose that the Darwinian mechanism of 'preadaptation' yielded an autonomous syntax module that had no counterpart in other animals. The evolutionary framework proposed by Charles Darwin has no problem in accounting for the evolution of patterns of behavior that involve 'new' biological substrates. Darwin in discussing the 'transitions of organs' proposed that 'an organ originally constructed for one purpose... may be converted into one for a...wholly different purpose.'... Evidence consistent with the process of preadaptation is found at all levels of biological inquiry (Mayr, 1982). The restructured organ often does not play any role whatsoever in its original function; for example, the bones of the mammalian middle ear play no part whatsoever in swallowing, although they are derived from bones found in the reptilian jaw. However, there is no general reason that prevents restructured organs from taking part in aspects of behavior that reflect their 'original' preadaptive role. Perhaps for this reason

Bloom vehemently rejects the theory proposed in Lieberman... that claims that the preadaptive basis of the brain mechanisms regulating syntax is speech motor control. Pinker and Bloom...instead claim that the human brain's syntax derived from some unspecified neural mechanism that they claim had no adaptive function whatsoever. It is impossible to test this theory since they do not specify the hypothetical nonadaptive neural structure involved; it is also difficult to conceive of any major component of the brain not having had some adaptive value in the past."

For the sake of those not involved in linguistic theory, fortunate few though you may be, we must stop and examine this word "module." Normally, it means "a set of things arranged in a particular way and designed to have a specific function." In an automobile, the engine is a module and so is the horn (klaxon). In this linguistic example, the set seems to be grammatical rules, and the function is to govern the formation and interpretation of sentences. Or something like that. Much the same thing has been called by various terms since the Chomskyite revolution began. One term was "language acquisition device (LAD)," another was "inborn capacity for language" or the ilk, and so forth. For several decades since Eric Lenneberg, we have had the notion that evolution through mutations have given us a native or inherent capacity to use human language, that we had such at birth, and that we used it to acquire whatever language we were exposed to in our childhood. And sometimes two or three languages at once. Now the little gem has been renamed the "autonomous syntax module."

On the way to his concluding paragraph, Lieberman has an interesting footnote (fn. 1, p. 772), in which this sentence occurs:

"A Universal grammar of the type postulated by Pinker...or Chomsky..., where every human child must possess an identical set of interlocking rules, parameters, and principles, is impossible given the universal nature of genetic variation..." Since such a grammar is so important to Chomskyites, we must stop and wonder if Phil's statement need be true. Can we not have modules which are universally highly similar or identical but also inherited? If they are vital, can not the death rate from harmful mutations keep the genetic variability in line? Having had a heart with a developmental defect in it, I suppose that human hearts as modules tolerate very little genetic variety — heavy functional load and all that. What do you all think?

Phil's last paragraph says that: "Any aspect of behavior that would contribute to biological fitness (the survival of progeny) involving prefrontal cortex would also contribute to syntactic ability... Since prefrontal cortex is involved in fine motor coordination and virtually all aspects of cognition, the evolution of human linguistic ability cannot [be] (— HF) disassociated from other adaptive behaviors as Bickerton... and Pinker and Bloom...propose. Nor is the saltation proposed by Bickerton..., restricted to neocortical structures, plausible given the involvement of subcortical pathways in the speech, syntax, and cognition deficits noted in

aphasia and PD. In other words, the evolution of the brain mechanisms that yield syntax cannot be dissociated from other aspects of human behavior. Likewise, the evolutionary history of these brain mechanisms accounts for their taking part in other linguistic and nonlinguistic aspects of behavior." End quote.

The "saltation" proposed by Bickerton would be an example of Stephen J. Gould's "punctuated evolution." Not gradual change as in Lyell's old uniformitarian model but evolution by leaps and bounds. Some key bibliography needs to be cited.

- Bickerton, Derek. 1990. *Language and Species*. Chicago, IL: University of Chicago Press.
- Mayr, Ernest. 1982. *The Growth of Biological Thought*. Cambridge, MA: Harvard University Press.
- Pinker, S. and P. Bloom. 1990. *Behavioral and Brain Sciences* 13, pp. 707-784.
- Savage-Rumbaugh, Susan, K. McDonald, R. A. Sevcik, W. D. Hopkins, and E. Rupert. 1986. "Spontaneous symbol acquisition and communicative use by pygmy chimpanzees (*Pan paniscus*)." *Journal of Experimental Psychology, General* 115, pp. 211-235.

Another paper on the same subject in *Brain and Language* 43, pp. 169-189 (1992), was entitled "Speech Production, Syntax Comprehension, and Cognitive Deficits in Parkinson's Disease." His co-authors were Edward Kako, Joseph Friedman, Gary Tajchman, Liane S. Feldman, and Elsa B. Jimenez. All are at Brown University, except one from Yale. We present the abstract without further discussion.

"Speech samples were obtained that were analyzed for voice onset time (VOT) for 40 nondemented English speaking subjects, 20 with mild and 20 with moderate Parkinson's disease. Syntax comprehension and cognitive tests were administered to these subjects in the same test sessions. VOT disruptions for stop consonants in syllable initial position, similar to those noted in Broca's aphasia, occurred for nine subjects. Longer response times and errors in the comprehension of syntax as measured by the Rhode Island Test of Sentence Comprehension (RITLS) also occurred for these subjects. Anovas indicate that the VOT overlap subjects had significantly higher syntax error rates and longer response times on the RITLS than the VOT nonoverlap subjects — $F(1,70) = 12.38$, $p < 0.0008$; $F(1,70) = 7.70$, $p < 0.007$, respectively. The correlation between the number of VOT timing errors and the number of syntax errors was significant. ($r = 0.6473$, $p < 0.01$). VOT overlap subjects also had significantly higher error rates in cognitive tasks involving abstraction and the ability to maintain a mental set. Prefrontal cortex, acting through subcortical basal ganglia pathways, is a component of the neural substrate that regulates human speech production, syntactic ability, and certain aspects of cognition. The deterioration of these subcortical pathways may explain similar phenomena in Broca's aphasia. Results are discussed in relation to 'modular' theories."

IN THE PUBLIC MEDIA:

The following 2 articles appeared in *Mammoth Trumpet*, vol. 8, no. 4, pp. 4-5 (1993). They are reprinted here with permission.

MOLLUSKS, NOT MAMMOTHS

The idea that people came to the Americas by way of an ice-free corridor is so widely accepted in both academic and popular circles that it is easy to forget that there are other hypotheses. To be sure, mammoth hunters and herds were widespread in North America between 10,000 and 12,000 years ago. But a significant body of research is suggesting that people may have migrated from Asia much earlier by way of the North Pacific coast.

Evidence presented by a variety of investigators is proposing that mollusks and other seafood, rather than mammoths and other large land mammals, first brought people east out of Asia. These scientists argue that:

- Environmental conditions were suitable for a coastal migration.
- Pleistocene-age people did possess seafaring technology to build boats and sail them across open water.
- The archaeological record provides evidence of extremely old sites that support the coastal-migration hypothesis.
- Dated stone tools show that ancient industries existed along the route.
- A growing chain of circumstantial evidence in linguistics, human biology, and ethnographic analogy supports the idea of a Pacific Rim migration.

If the first settlers were prodded not by the movements of mammoth herds but by an abundance of clams, mussels, crabs, fish, sea birds and sea mammals, they would have progressed around the Pacific Rim as sea levels and tides permitted. They likely used some kind of boats, and their progress would likely have been blocked for periods by glacial ice. They moved initially north out of Asia then south out of Beringia. Unfortunately, the likelihood of finding archaeological records of coastal migration is slim because of post-glacial rises in sea level, and that makes the coastal hypothesis unpopular with archaeologists. The coastal hypothesis also suffers because the earliest cultural pattern recognized and accepted by many archaeologists is based on the hunting of big-game mammals and is characterized by a tool kit containing fluted projectile points. Such points have not conclusively been found on the possible route along the coast of British Columbia or Alaska — at least none that is contemporary with, or older than, those found in mid-continent that date to an inland late-entry hypothesis benchmark year of about 11,000 years B.P.

Searchers may never find the coastal sites. However, the archaeological, linguistic, ethnographic, and biological evidence supporting the Pacific Rim hypothesis is growing. Although mostly circumstantial, the evidence points toward peopling of the Americas by a coastal migration that Simon

Fraser University archaeologist Knut R. Fladmark contends was environmentally possible anytime during the past 60,000 years.

In papers published more than a decade ago, Fladmark rejected an inland corridor route of migration. Basically, he said the southern extension of Beringia was little more than an inhospitable and constantly shifting swamp of ice water during various stages of Pleistocene glacial activity. As early as 1979, he suggested that the Pacific Northwest Coast had environmentally hospitable havens of ice-free land that could have provided food supplying way stations for southbound Paleoindian migrants. And in 1990, Fladmark wrote that radiocarbon analyses of buried plant material overlying glacial till deposits in a deep channel at Cape Ball near the northeastern end of British Columbia's Queen Charlotte Islands "indicate minimum ages for local deglaciation and establishment of a terrestrial and wetland plant community" at about 16,000 years B.P. In 1979 Fladmark wrote in *American Antiquity*:

There is no evidence that the North Pacific became permanently frozen during glacial episodes, although seasonal freezing of sheltered waters seems likely. The Japanese Current would have continued to bring warm subtropical water masses along the outer edge of the continental shelf, undampened by any Arctic flow through Bering Strait, and mean annual temperatures at sea level were probably above freezing.

Fladmark is not alone in seeing the coastal route as more hospitable to Paleoindian migrants than an interior route. Although saying that evidence to support a coastal route is far from conclusive, University of Oregon archaeologist Jon Erlandson gives his "visceral" opinion...that life along the coast had to be better than spending a winter in the "freezing, dark and forbidding landscape of Beringia's interior." And Ruth Gruhn, of the University of Alberta, found that Paleoindians moving down the coast during the middle Wisconsin period between 60,000 and 30,000 years ago would have encountered environmental conditions similar to those of today. Migrants would also have found a rich variety of shellfish, fish, and migratory waterfowl.

Caribou that live on Queen Charlotte Islands today also suggest to Fladmark an ancient survival of cold-adapted land mammals on an island that post-glacial flooding put beyond the range of their swimming ability. Human coastal migrants would have preyed on such mammals as part of their subsistence base. "The Queen Charlotte Islands," Fladmark wrote in 1990,

would seem to represent a particularly important 'stepping stone' along any coastal route of migration for early people moving south from Beringia, despite their presently isolated location. Indeed, 'the Charlottes' are currently the first area in Canada south of Beringia for which there is incontrovertible evidence for the existence of a terrestrial plant community, theoretically capable of supporting some animal and perhaps human life, during the peak of the last glacial period.

However, Gruhn has noted in a recent publication that it might be difficult to identify an exact middle-Wisconsin coastline along the North Pacific, largely because of local uplifting of land. She says that deep-sea core samples from the Bering platform suggest that the middle-Wisconsin phase was the best time for human movements along the south edge of the Bering Land Bridge. That environment, analysis of the core sample indicated, would have consisted of winter sea ice, with thawing in the spring uncovering a productive marine ecosystem capable of feeding people. And farther south, the environment only would have become more hospitable.

Shellfish Low in Calories

There is little doubt that, if the review by Fladmark and Gruhn of coastal environmental conditions is correct, early Americans would have used all the food sources available to them. Archaeologist David R. Yesner, in discussing the prehistory and ecology of maritime hunter-gatherers, has noted 150,000-year-old shell-midden evidence of marine foods as a central subsistence focus in South Africa. However, Yesner also observes that shellfish diets are notoriously low in calories and would not provide an adequate diet in a cold climate. Indirectly, that lends support to the need for Pleistocene coastal hunters to augment a water-based food supply with meat, as Fladmark suggests may have happened where caribou probably occurred on the Queen Charlottes. It also bolsters Fladmark's contention that the "classic" big-game hunting tradition associated with fluted points might also have developed out of earlier cultural patterns adapted to hunting on coastal refuges. Fish, sea birds, and sea mammals could just as easily have added to humans' meat supplies.

Extensive analysis by Erlandson of shell middens on the California coast and offshore islands shows that by 10,000 years ago, and possibly earlier, people had adapted to a marine-subsistence economy.

Yahgan People as an Analogy

To establish a case for adaptability of a coastal people to live in a middle-Wisconsin high-latitude coastal environment, Gruhn turns to ethnographic analogy of the Yahgan Indians of coastal Tierra del Fuego. Observed as early as 1578 by Sir Francis Drake, by Charles Darwin in 1832, and in the twentieth century by others, the Yahgan people were lightly clothed and lived in stick huts in a stormy environment that sometimes includes snow in summer. Gruhn also notes that they hunted, fished, and captured birds with a meager tool kit that included bone points on wooden spears, pronged sticks, mussel-shell knives, wooden clubs, fiber snares, and stone-tipped arrows. They also used canoes made of several strips of bark cut from beech trees with a bone chisel or mussel-shell knife. The strips were sewn together with lashing of whalebone or shredded saplings. The canoes leaked badly, Gruhn reports, but were adequate for the frequent movement of families along the coastline. As she wrote for the forthcoming book, *Method and Theory for Investigating the Peopling of the Americas*:

One could surmise that even a population as poorly endowed with material culture as the ethnographic Yahgan could have made it into the New World along the North Pacific coast during the middle Wisconsin interval.

Erlandson suggests that exploitation of coastal resources 13,000 years ago might provide another circumstantial tie to a coastal route of migration — if the near-coastal site at Monte Verde in Chile withstands careful scrutiny. Erlandson notes that Monte Verde is about 50 km from the Pacific coast and contains trade resources such as salt, although the trade link remains to be established.

The Evidence for Boats

Boats of some sort seem mandatory for people to have lived and moved along the shore. So, too, would be the seafaring knowledge necessary to ply a frigid and dangerous Beringian seacoast. No Pleistocene-age boats have yet been found. And authorities such as Jesse D. Jennings have declared that open-water voyaging capabilities were not known until thousands of years later than Pleistocene people would have needed them for such trips. Although Erlandson has reported that maritime peoples lived in California, British Columbia, and southeast Alaska as early as 10,000 years ago, circumstantial evidence must be used to arrive at earlier dates. But that evidence offers some interesting possibilities.

Recent data from Greater Australia strongly suggest that boats were used to colonize the southern end of New Ireland from New Guinea by way of New Britain about 33,000 years ago. That evidence is based on analysis of shell-midden material, obsidian from New Britain, and on faunal remains. Similar evidence puts humans in a rockshelter on Buka Island in the Solomons 28,000 years ago. Ocean voyages of up to 80 km would have been required to reach those islands — strongly implying a firm grasp of seafaring knowledge — because the only way to get there would have been by boat.

The Pleistocene North Pacific would have presented different problems for mariners from those faced by seafarers in the warm waters of the South Pacific, but archaeologists have been finding evidence that Paleolithic peoples there were moving across open water near Japan at least by 30,000 years ago. Shizuo Oda has reported on archaeological evidence that people used seagoing boats to obtain obsidian from Kozushima Island. The island — about 170 km south of Tokyo and about 54 km from Shimoda on the Izu Peninsula — appears neither to have been covered by glacial ice nor connected by a land bridge to mainland Japan during Pleistocene glaciation; boats would have been necessary to transport that toolmaking material.

...

THE CASE FOR A PACIFIC RIM MIGRATION

GEORGE WISNER

“On the Japanese mainland,” Oda writes in the book *Man and Culture in Oceania*,

the Kozushima obsidian is found in Paleolithic and Jomon sites on the Musashino Upland, where it is identified in Paleolithic sites as old as 30,000 B.P., and in Jomon sites as far as 200 km from the source. Significantly, even during the late Pleistocene, when sea levels were 100-140 m. lower than today, Kozushima was separated from the Izu Peninsula by a wide strait of water, making it impossible to acquire Kozushima obsidian without the use of dugout canoes or rafts. The very early use of obsidian from the Izu Islands shows that Paleolithic peoples in Japan had already developed the means to travel across water, settling the base for the later highly developed water transport technology of the Jomon.

The earliest Jomon culture dates to 11,000 to 13,000 B.P.

Stepping Stones to the New World

In a 1991 paper on the origins of Japanese Paleolithic, archaeologist Charles T. Keally says, “It is not known when humans in eastern Asia acquired the capability to cross large bodies of water.” But, he says that “humans of the southern Chinese type” were on the island of Okinawa roughly 30,000 years ago “when that island was probably separated from the continent.” The statement, in context, is designed to support his contention that people were on Japan no earlier than 35,000 years ago. Although no Paleolithic boats have been found, the combined circumstantial evidence surrounding the obsidian mine strongly suggests that there was very early boat travel in the North Pacific — early enough to make possible a hypothesized boat voyage to the New World.

Erlandson suggests that at the height of the last glaciation, the Kuril Islands, which form a crescent north and east of Japan toward the Kamchatka Peninsula, could have been stepping stones for Paleolithic people going from the Japanese archipelago to the south shore of Beringia — and then possibly south through the Queen Charlottes. This suggestion echoes those of Fladmark and Gruhn.

Although many archaeologists remain skeptical of the seafarers' route to the New World, Fladmark has no problem defending the idea of a sea passage along the Northwest Coast. “Given any kind of steerable watercraft, [people's] ability to reach the sea-level refugia of the North Pacific from Beringia seems undoubted. The only difficult area is the Pacific Coast of the Alaska Peninsula west of Kodiak Island, where there is no direct evidence, as yet, for any unglaciated refugia,” he wrote in a 1979 paper...

If coastal migration by boat can be seen as possible, it also can be seen as perhaps the most rapid method to settle fully the coastal areas of the New World and ultimately push human culture inland. That idea collides with the implications inherent in Paul S. Martin's prehistoric-overkill hypothesis. An element of that hypothesis suggests that a small group of Paleolithic hunters armed with new technology — fluted stone spear points — coupled with population growth allowed them to move rapidly south through Beringia about 12,000 years ago, reaching the tip of South America approximately 1,000 year later. Fladmark calls that slow. "Theoretically," he has written, "even primitive boats could traverse the entire Pacific coast of North and South America in less than 10-15 years." Such rapid southward movement, coupled with an early entry into the New World, would help explain sites in South America that predate the 12,000-year-old late-entry model favored by many North American archaeologists.

Part of the Pacific Rim hypothesis assumes Paleolithic migrants came from Asia, not elsewhere. Although few North American sites contain human fossil remains, D. Gentry Steele and Joseph F. Powell have analyzed available skeletal remains ranging from 8,500 to 10,000 years old and found that the closest affinities are with Asian populations. (See *Mammoth Trumpet* 7:2, "Paleoindian Skeletal Data Re-examined.") Comparisons of recent American Indians with other populations indicate that American Indians are most similar to Asian populations, most notably northern Asians.

Arguments for Early Arrival

The timing of the colonization of the Americas also is an issue with supporters of a Pacific Rim hypothesis. Many such as Gruhn argue that paleolithic people arrived in the New World considerably earlier than the 12,000 years ago allowed by "late arrival" theorists.

Hard archaeological evidence to support a Pacific Rim hypothesis is scarce, but various circumstantial methods have been used to estimate timing of the arrival in the New World. Linguistics is one technique. Ruth Gruhn regards linguistics as concrete evidence supporting the Pacific Rim peopling of the Americas. In a 1988 publication, she examines the distribution of aboriginal languages and concludes that there is "great linguistic diversification [that] implies great time depth for human occupation of the Pacific Coast, the Gulf Coast, Central America and South America." It has been estimated, she says, that there are more than 1,500 native languages in South America alone; from 200 to 350 known languages in Mexico and Central America; and all but one of seven language groups identified on the Northwest Coast are considered independent languages. Such language diversity does not exist along the suggested inland migration routes, she adds, and therefore linguistics supports her contention that seafarers first reached the continent and moved rapidly south.

Genetic Markers Indicate Early Migration

Some scholars also are turning to the biological record in an effort to determine when people began coming to the new world. Among these is Moses S. Schanfield, whose analysis of genetic markers of the GM-AM system on heavy-chain immunoglobulin (a protein antibody) from living American Indian people indicates that their ancestors arrived in the New World in four different migrations... "The best estimates are that the first migration occurred before the major Wisconsin glaciation in the period 17,000-25,000 B.P.," he says. Schanfield stops short of offering a possible route of entry for the migrants. But his study does appear to fortify arguments that people of Asian descent have been in the New World far longer than the 12,000 years suggested by the "late entry" model, which posits migration by way of an inland route south from Beringia.

Dated archaeological sites are considered to be the most direct evidence for establishing how early people arrived in the Western Hemisphere. If people came through a mid-continent ice-free corridor, it would follow that the oldest sites should be found in the north rather than in the south. But at the present time, the oldest firmly dated sites for the Americas come from two areas in South America — Pedra Furada in northeast Brazil and Monte Verde in Chile. Dates as old as 20,000 years have been reported by Gruhn for sites in Mexico, where extinct fauna such as camelids, horse, and mastodon have been found in association with lithic artifacts.

Incontestable proof of migration to the Americas by way of the Pacific Rim remains elusive and may never be found, but dismissing it offhand also is becoming more difficult in light of the growing archaeological, linguistic, biological, and circumstantial evidence being used to champion a position that Fladmark and others have been trumpeting for more than a dozen years. As Paul Martin has reminded readers: "Absence of evidence is not evidence of absence."

Like other hypotheses, the Pacific Rim hypothesis may be proved or disproved by the testing of hypotheses that can be derived from it. Gruhn sees at least two predictions she believes could settle the issue:

The model would be supported if an archaeological site of middle Wisconsin age is demonstrated in western Oregon, California, or Mexico. The model would be discredited if an archaeological site dated 50,000 years B.P. or older is discovered on the northern Great Plains, at the southern end of the Ice Free Corridor.

The coastal-route hypothesis offers a scenario for a peopling of the Americas that is tantalizingly different from that of the heavily armed hunters clad in mammoth skins trudging down a windswept Beringian landscape inland toward South America. Perhaps the quarry was mollusks, not mammoths.

The following article is reprinted with permission from *Science News*, vol. 144, p. 380 (4 December 1993).

BRONZE AGE CHARIOTS ROLL BACK IN TIME

While conducting research in several former Soviet republics last year, David W. Anthony and Dorcas R. Brown of Hartwick College in Oneonta, N.Y., met a Russian archaeologist who told them about some remarkable finds. His research team and several others had uncovered the remains of chariots placed in graves from a culture that flourished in the steppes along the Russia-Kazakhstan border about 4,000 years ago.

New radiocarbon dates for bone samples taken from horse skulls in one of these graves — at an excavation directed by that same scientist, Nikolai B. Vinogradov of the Chelyabinsk State Pedagogical Institute — range between 2200 B.C. and 1800 B.C., making the associated chariots the oldest such vehicles preserved anywhere, Anthony reports.

This evidence does not, however, resolve a long-standing scholarly dispute about whether chariots first emerged in the Eurasian steppes or in the Near East, Anthony notes. Clay impressions of chariots found at a Turkish site date to as early as 1950 B.C., making them nearly as old as the Russian finds.

"The complex carpentry involved in chariot making suggests that this [type of] vehicle was invented in one place and then rapidly diffused elsewhere," Anthony contends. "I'm leaning toward the steppes as the chariot's place of origin."

In the last decade, Russian and Kazakh archaeologists have uncovered at least 25 fortified sites belonging to this Bronze Age culture. Cemeteries at these settlements contain graves that have yielded pieces of wheels and spokes from 14 chariots, Anthony reports.

Chariot wheels were found fitted into slots on the floors of the graves. A distance of about 3.5 feet between the vehicles' two wheels suggests that they carried only one person, according to Anthony. Ancient inhabitants of the region may have devised chariots as high-speed platforms from which warriors could shoot arrows or hurl spears, he maintains.

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The following letter, dated 30 September 1993, was received from David W. Anthony — it bears directly upon the points discussed in the above article.

Sorry to have been so late with my dues payment for ASLIP. It's been a busy summer. You might be interested to know that I am now working on a piece about chariot burials of the Sintashta-Petrovka culture in the steppes east of the Ural Mountains. Sintashta-Petrovka is now represented by about 20-25 settlements with timber-reinforced earthen fortifications and associated kurgan cemeteries distributed across the northern steppes between the Ural foothills and the Ishym River. I think it very likely that the Sintashta-Petrovka culture is the material

remnant of the speakers of *proto-Indo-Iranian*. The chariots discovered in some 14 Sintashta-Petrovka graves are the oldest physical remains of chariots anywhere in the world, and they might represent the invention of chariot technology in the steppes. (Was chariot technology invented by steppe barbarians rather than in the Near East? There's a lot of resistance to this idea, but Piggott would like it.) I recently brought back a sample of horse bone from two horses buried with a male and a chariot with 10-spoked wheels and a couple of Sintashta pots at the site of Krivoe Ozero, near Troitsk; four accelerator radiocarbon dates from these horses calibrate to 2100-1900 BC. Russian archaeologists generally agree that Sintashta-Petrovka was the base from which grew the Andronovo horizon, the first cultural community to extend across the entire steppe zone from the Urals to the Tien Shan. The Indic-speaking Mitanni probably detached from some southern variant of Andronovo. So Sintashta-Petrovka might represent the ancestral Indo-Iranian community. In terms of elaborate horse and cattle sacrifices, human sacrifices at the graveside, chariots, fortified settlements, and fire rituals, it fits pretty well. Details will be forthcoming...

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The following article is reprinted with permission from *Science News*, vol. 144, pp. 196-197 (25 September 1993). It presents another view of the work of Harpending et al. (cf. pp. 43-49 of this issue).

NEW GENE STUDY ENTERS HUMAN ORIGINS DEBATE

In a finding that captured the imagination of scientists and the public alike, researchers announced in 1987 that an analysis of mitochondrial DNA — genetic material located outside the cell nucleus and inherited only from the mother — traced the maternal lineage of all humans back to an African "Eve" who lived about 200,000 years ago. A computer-run statistical analysis of mitochondrial DNA samples drew an evolutionary tree with African roots lying at mitochondrial Eve's feet, suggesting modern humans originated in Africa and rapidly spread elsewhere.

Fatal statistical flaws in this approach later emerged and researchers dropped genetic arguments for the "Out of Africa" theory (SN: 2/22/92, p. 123). However, a new type of mitochondrial DNA analysis, described in the August-October *Current Anthropology*, now presents a more complicated picture of human evolution.

In this scenario, a small subgroup of *Homo erectus* evolved into modern humans — probably in Africa — and slowly trekked to several parts of Europe and Asia beginning around 100,000 years ago. About 50,000 years later, geographically isolated human populations experienced dramatic growth and expansion fueled by the appearance of many cultural innovations.

Genetic analysis offers weaker support for the multiregional evolution theory, a competing view of human

origins, contend Henry C. Harpending, an anthropologist at Pennsylvania State University in University Park, and his colleagues. The multiregional theory holds that modern humans evolved simultaneously in several parts of the world for around 2 million years, with contact between separate populations along the way.

Rather than constructing evolutionary trees out of genetic data, Harpending's group analyzed the differences in sequences of mitochondrial DNA both within and between human groups now living in Africa, Asia, and Europe. According to the researchers, these differences preserve a record of ancient population expansions and separations, which they modeled in computer simulations of mitochondrial DNA change in pairs of populations.

"In living populations, between-group mitochondrial DNA differences far outpace within-group differences," Harpending holds. But this pattern of change in the structure of mitochondrial DNA does not characterize his computer models of single populations that rapidly grow and split into separate clusters. "Groups of archaic humans apparently remained isolated from each other for tens of thousands of years," Harpending says.

The dating of humanity's common mitochondrial ancestor does not show that our species suddenly evolved around 200,000 years ago, Harpending says. The mitochondrial DNA evidence simply cannot illuminate the structure of human populations before that time, he asserts. But his group estimates that the number of human females at the time mitochondrial Eve lived ranged from 1,000 to no more than 10,000.

This relatively small population shows genetic signs of slight size expansion in Africa around 100,000 years ago, with major size increases occurring on that continent approximately 80,000 years ago, the researchers maintain. Population growth blossomed in Asia and Europe about 50,000 to 40,000 years ago, according to the mitochondrial DNA comparisons.

Up until these growth spurts, stone tools and other artifacts found at sites throughout Eurasia displayed many similarities; soon thereafter, sophisticated regional cultures appeared, Harpending and his co-workers note. Indeed, cultural change may have sparked marked population increases in dispersed human groups, they argue.

Alan R. Templeton, an evolutionary biologist at Washington University in St. Louis, regards the new analysis of mitochondrial DNA with considerable skepticism. He provided the statistical critique that chopped down earlier evolutionary trees derived from mitochondrial DNA.

"This study is a step in the right direction," Templeton remarks. "But the computer models of population expansion are pretty simple and only test the Out of Africa theory, not multiregional evolution."

Harpending acknowledges that large margins of error exist in his simulations: "We all feel that we need to move beyond mitochondrial DNA as a locus of study."

In a report in the March *American Anthropologist*, Templeton found no evidence for a definite geographic origin for a common mitochondrial ancestor, whom he dates to around 800,000 years ago. Current mitochondrial DNA

variations come from dispersed, ancient populations, he contends.

Using a computer program that analyzes the geographic distribution of DNA differences, Templeton concluded that humans experienced size expansions largely within continents, with periodic contact across continents.

— B. Bower

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The following article is reprinted with permission from *Science News*, vol. 144, p. 277 (30 October 1993).

FOSSIL JAW OFFERS CLUES TO HUMAN ANCESTRY

Investigators have uncovered a lower jaw in Africa that represents one of the earliest known fossils of a direct human ancestor, or member of the genus *Homo*. The specimen, assigned a preliminary age of about 2.4 million years by its discoverers, turned up in fossil-poor sediments near Lake Malawi, roughly half-way between sites in eastern and southern Africa that have yielded the bulk of ancestral human remains.

Another *Homo* fossil may also date to 2.4 million years ago (SN: 2/29/92, p. 134).

The *Homo* lineage apparently originated in the tropics of eastern Africa around 2.5 million years ago, following a gradual shrinkage of habitable land to the south and north caused by global cooling, assert Friedemann Schrenk, a paleontologist at Hessisches Landesmuseum in Darmstadt, Germany, and his colleagues. The species they assign to the new find, *H. rudolfensis*, eventually migrated as far south as Lake Malawi, they state. Another species, *H. habilis*, traveled even farther south between 1.8 million and 1.5 million years ago as temperatures climbed, they argue in the Oct. 28 *Nature*.

"Many scenarios of human evolution are still possible," contends Timothy G. Bromage, an anthropologist at Hunter College in New York City who participated in the Malawi excavations. "But this new fossil looks like the large-toothed, large-jawed specimens from eastern Africa that some researchers call *Homo rudolfensis*."

Microscopic study of tooth enamel shows a pattern of dental development in the jaw similar to that of proposed *H. rudolfensis* fossils, Bromage adds.

The leading proponent of *H. rudolfensis* as a bona fide species, anthropologist Bernard Wood of the University of Liverpool in England, supports Bromage's classification of the fossil jaw. Given the small number of early *Homo* fossils and disagreements over how to group them (SN: 6/20/92, p. 408), other scientists decline to assign the jaw to a specific species.

Schrenk has directed the Hominid Corridor Research Project at sites in southeastern Africa since 1983. These excavations have attempted to clarify how animals and hominids, or members of the human evolutionary family, evolved and moved through that part of the continent.

The Lake Malawi site does not contain an abundant trove of fossils. Beach sand propelled by powerful waves on the lake has scoured out much fossil-bearing sediment, Bromage notes. In the past decade, field workers have unearthed about 600 animal bones, mainly from pigs and antelopes. Last year, two joining parts of the hominid's lower jaw appeared. Many teeth remained in their sockets.

For now, Bromage considers the age estimate of 2.4 million years, based on previously known ages of animal bones found near the jaw, to be a best guess. Investigators have not found any datable volcanic ash in the hominid-bearing sediment, but measurements of magnetic orientations in the deposits may eventually produce a more accurate estimate, he says.

The jaw indicates that *H. rudolfensis* evolved a face specialized for chewing, much like that of *Paranthropus*, a smaller-brained hominid lineage that lived at the same time, Wood argues. The find also supports the view that hominids and other mammals at the Malawi site maintained stronger links to eastern, rather than southern, Africa, he asserts.

Tim D. White, an anthropologist at the University of California, Berkeley, says the jaw may represent either a large male *H. habilis* or a *H. rudolfensis* of undetermined sex. Early *Homo* species may have experienced large fluctuations in body size over time, making it difficult to identify them from fossil remains, White contends.

He calls the age assigned to the new specimen "speculative." Still, the jaw shows that early *Homo* species extended south about 1,000 miles from eastern Africa and bore "striking similarities" in anatomy, White holds.

— B. Bower

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The following article is reprinted with permission from *Science News*, vol. 145, p. 5 (1 January 1994).

NEANDERTAL TOT ENTERS HUMAN-ORIGINS DEBATE

Around 60,000 years ago, one or more Neandertals buried a dead 10-month-old infant in a cave in northern Israel. Before filling the grave with dirt, someone placed the jawbone of a red deer against the baby's hip in a gesture that apparently held symbolic meaning.

That, at least, is the scenario presented by scientists who unearthed the infant's remains in 1992 at the Amud cave near the Sea of Galilee. Their analysis of the fossil, set to appear later this year in the *Journal of Human Evolution*, supports the view that Neandertals inhabited the Middle East along with *Homo sapiens*. It also suggests that Neandertals possessed enough unique skeletal traits to exclude them from playing any role in the evolution of modern humans.

"The exciting thing is that we can identify a Neandertal infant based on anatomical structures outside the midfacial region," asserts Yoel Rak, an anatomist and paleontologist at Tel-Aviv University in Israel. Neandertal

midfacial bones portray sloping foreheads, swept-back cheeks, and projecting jaws.

Only the lower jaw, skull base, and several cranial bones remain in good shape on the Israeli specimen, report Rak and his co-workers, William H. Kimbel, an anthropologist at the Institute of Human Origins in Berkeley, Calif., and Erella Hovers, an archaeologist at Hebrew University in Jerusalem. The vertebral column and ribs also survived the millennia, as did incomplete pieces of the pelvis and other lower-body bones.

The Amud infant displays three features unique to Neandertals, Rak argues: a chinless lower jaw; an oval-shaped hole in the base of the skull, called the foramen magnum, through which the spinal cord passed; and a bony lip at the back of the lower jaw, on the inner surface, where an important chewing muscle attached.

From below, the Amud jaw shows a "squarish" profile, indicating the lack of a chin, Rak contends. A similar profile characterizes older juvenile Neandertal jaws found in Eastern Europe and the Middle East, he says. In contrast, fossils of anatomically modern children found at Israeli sites from the same period contain triangular lower jaws, signifying the presence of a chin, he says.

The oval foramen magnum of the Amud specimen also departs from the rounded shape of this feature in living humans and most other primates, Rak adds. Four other partial skull bases of Neandertal children found elsewhere show an oval-shaped foramen magnum, he maintains.

It remains unclear whether this trait reflects any major differences in the workings of the Neandertal spinal cord and central nervous system.

The third clue to the fossil baby's species comes from bony protrusions for a chewing muscle known as the medial pterygoid. These bumps along the jaw's inner surface get larger toward the back of the mouth. Thus, the muscle thickened as it moved up the lower jaw, Rak holds.

The muscle markings end at a bony lip, which served as an anchor for the medial pterygoid, the Israeli researcher notes. The same feature occurs on other Neandertal fossils but not on fossil or modern *H. sapiens*, he asserts.

The function of a thick medial pterygoid muscle at the back of the mouth eludes Rak. In fact, it contradicts his prior theory that Neandertals chewed their food most vigorously with their front teeth.

Still, Neandertals apparently passed these three traits on genetically, since the features appear even in an infant, Rak argues. Only a species distinct from *H. sapiens* could display these and other unique structures, he adds.

Controversy over Neandertals in the Middle East continues, however (SN: 6/8/91, p. 360). Some researchers, such as Fred H. Smith of Northern Illinois University in DeKalb, welcome the new find, yet still class Neandertals and early modern humans in that region as closely related subspecies. Others place the two groups in a single population of "archaic" *H. sapiens*.

— B. Bower

LETTERS TO THE EDITOR

In two recent issues of *Mother Tongue* Hal Fleming has commented negatively on publications of mine critical of the work of Joseph Greenberg and Merritt Ruhlen. I write in response to these comments.

In the first case, regarding my Book Notice on Merritt Ruhlen's *Guide to the Languages of the World* in *Language* 69.1.220-222 (1993), I observe that he offers no substantive commentary whatsoever, but merely urges me to examine my own biases. Apparently, he believes that the mere fact that I am critical of Ruhlen's book shows that I must be irrational. I submit that any objective observer, regardless of his or her views on historical methodology, would draw the same conclusion I did, that Ruhlen's book is "incomplete, misleading, and inaccurate". Let me explain why.

What is required to demonstrate that two languages are genetically related? First, we must show that there are congruences between them that cannot be attributed to chance or to universals. When we have done this, we have demonstrated a historical connection but do not yet know its nature. Second, we must show that the historical connection is not due to diffusion. These are logical steps, not procedural ones, for we can often eliminate some loans from consideration before attempting to establish non-chance congruences.

As far as I can see, there is no real disagreement regarding what I have just written. That is, everyone agrees that one must rule out chance, universals, and diffusion in order to make a good argument for genetic affiliation. What is subject to dispute is what sort of evidence it takes to rule these out. In particular, there is controversy over the likelihood of chance similarities and over the frequency of borrowing of morphology and various types of "core" vocabulary.

Now, insofar as Ruhlen's book purports to present a serious discussion of historical methodology, it is incumbent upon him to discuss these issues, whatever positions he may take on them. He does not do so. The problem of chance similarity is simply not discussed. Diffusion is almost completely ignored. The only discussion is a brief reference to François Grosjean's book on bilingualism to the effect that bilinguals do not borrow extensively from one of their languages into the other. There is no discussion whatever of the various cases in the literature of language mixture, massive borrowing, borrowing of morphology, and borrowing of "core" vocabulary. Consequently, it seems to me to be hardly a matter of opinion that Ruhlen's book is incomplete as a treatment of historical methodology. Regardless of what views one may hold on these questions, Ruhlen simply fails to address them.

As to Ruhlen's book being misleading and inaccurate, I will limit myself here to two telling examples. The book presents Greenbergian methodology as clearly successful and mainstream, in spite of the fact that it is extremely controversial and that most of his work is overwhelmingly rejected by historical linguists. Whether or not one agrees with Greenberg's methods and results, this is misleading.

Another example is Ruhlen's treatment of the exchange in *Current Anthropology* between Cavalli-Sforza et al. and Bateman et al. on the correlation between linguistic and

genetic classifications. In place of substantive comment, Ruhlen resorts to an *ad hominem* argument, claiming that Bateman et al. have admitted to knowing nothing about human genetics, quoting them as saying that they are "unfamiliar with the data, methods, and models of human population genetics". His evidence is a quotation from their rejoinder to Cavalli-Sforza's reply to their critique (Bateman, Richard, Goddard, Ives, O'Grady, Richard, Funk, V. A., Mooi, Rich, Kress, W. John, & Peter Cannell (1990) "On Human Phylogeny and Linguistic History: Reply to Comments." *Current Anthropology* 31.2.177-183.) This "admission" is taken seriously out of context. Here is the full passage:

We plead guilty to Cavalli-Sforza et al.'s anthropocentric charge that we are "biologists...unfamiliar with the data, methods, and models of human population genetics." We operate on the assumption that *Homo sapiens* is a biological species and that there are no methods or models peculiar to human population genetics. Methods and models of eukaryotic allogamous population genetics are applied with equal validity to humans and fruit flies. (p. 177)

As the quotation marks indicate, the original source of the portion quoted by Ruhlen is not Bateman et al. but Cavalli-Sforza et al. Bateman et al. are clearly not admitting to ignorance but are sarcastically quoting their opponents' words in denial. "Misleading" is if anything too mild a term for such egregious out-of-context quotation and misrepresentation.

In the second case, regarding my paper "The Salinan and Yurumangui Data in *Language in the Americas*," *International Journal of American Linguistics* 58.2.202-229. (1992), Fleming urges that someone refute me while admitting that he HAS NOT READ my paper. The mind boggles at how he can have formed the opinion that it is wrong and can and should be refuted without reading it. I submit that this is a prime example of the cultish mentality that pervades long-range work and the pages of MT. Discourse that relies on *ad hominem* arguments and team spirit is unhealthy and unscientific. Only if they are able to present solid data and substantive arguments will proponents of distant genetic affiliation have an impact on the scientific community.

William J. Poser
Stanford University

(A reply to this letter will appear in *Mother Tongue* 22!)

CORRECTION

Joseph H. Greenberg would like to append the following note of correction to his translation of Liedtke's review of *LIA* published originally in *Anthropos*. On page 39, line 25 replace *i* by *či* before Ticuna. In addition, Liedtke himself in the review omitted Cf. before the Ticuna citation, which is also crucial. A reader who wishes to check back to *LIA* pp. 192-3 will also see

that this is a well-supported etymology which occurs widely in nine of the 11 subgroups of Amerind and that the citation in Liedtke omits crucial semantic connecting forms which lend support to the validity of the etymology as a whole.

GUEST EDITORIAL

HAROLD C. FLEMING

Boston University / University of Pittsburgh

Speaking for the many people who have recently praised the improvements in *Mother Tongue*, let me express (for all of them and myself) congratulations to Allan Bomhard for the good job done. Being editor is a thankless job, as Terrence Kaufman pointed out long ago, so all the more reason to thank Allan for his hard work and skill and good heart. Merci, notre ami!

The previous editor wishes to apologize for insufficiently appreciating the contributions of Roger Wescott to our joint endeavors. After all, Trombetti was resurrected and lionized — to the embarrassment of one Italian colleague —, while Pedersen, Swadesh, and Illič-Svityč have each begotten prehistory cults. Greenberg is hero to many of us, and the devil incarnate to others. Even Karl Bouda has been appreciated; he did a lot, and Schuhmacher was right to point out his pioneering efforts. But Roger has been like Gregor Mendel and Leonardo da Vinci in having the brilliant ideas before the world was ready for them. Kroeber once wrote a long chapter on the fate of inventions “before their times had come.” The whole syndrome applies to Roger.

Roger is polite, kindly, and courteous. So he has not complained about being unrepresented in our Hall of Ancestors. Yet I can remember a time in the 1970s when Roger was the only one talking about long range comparisons, at least in the English-speaking world. (If I have overlooked any others, forgive me, but tell me their names.)

Thus, instead of presenting a distorted view of his innovations in linguistics — the what and why questions —, we will ask Roger to examine his memory and dredge through his old publications and tell us about this phase of his career. None of the other pioneers have been asked *why* they broke out of the prevailing paradigm, have they? Roger? Maybe we can even have it for MT-22 in the Spring of 1994?

We have four primary topics to discuss in this space. Three of them are comments on the contents of this issue. One is on a more general topic.

- homing in on PIE / p-IE / proto-Indo-European
- why be threatened by Colarusso's hypothesis?
- what to make of the Nostraticists of Michigan?
- long range theory / theory of prehistory : dating and locating or where? and when?

Homing in on PIE

Four strong articles on the general circumstances of PIE and its daughters and their movements are contained in this issue. Some of David Anthony's reporting on early IE movements escaped this issue; we should get it for MT-22. But two of his pieces are discussed along with Mair's findings. Victor Mair's report on Xinjiang (Sinkiang) is so exciting that we all wanted to go to press two months ago! Not only mummies of “Nordic” folk — and that's the judgment of Chinese archeologists — but also wheels, weaving and, DNA. All dated too. Also the surge of Chinese interest in these prehistoric things is *very* encouraging. Bravo, Victor!

That these Xinjiang people of the time of the Mycenaeans (and earlier) were north European in appearance, rather than looking “Mediterranean” as one sometimes hears, is very interesting. That these people eventually died out in Sinkiang, to be replaced by “Mongoloids,” is puzzling. That two distinct IE languages, called Tocharian A and Tocharian B, died out in the same area leads a normal human mind to identify Tocharian with the Nordics. Of course, it ain't necessarily so. But such is the heart of circumstantial evidence and, typically, such reasoning is correct. (For the general reader one is informed that Sinkiang is the older spelling for what outsiders used to call Chinese Turkestan; the large area is awash in Altaic speakers and Chinese administrators, and has been for close to a millennium.)

What makes the Tocharian connection so exciting is that it virtually points to its own tracks back to their starting point. Brief appraisals of maps of central Asia / west Siberia show the great steppe of Kazakhstan linking northern Sinkiang and the steppe country east of the southern Urals. There, around Kurgan and Troitsk, we find more archeological evidence of Indo-European, either Tocharian as I suspect or Indo-Iranian as David Anthony thinks. We will leave it to him to describe for us what is there. But when Victor heard about David's finds, he gave a great whoop and said that the steppe between the two areas was like a highway, especially for horse-riding pastoralists. He knew because he had taken a bus along that very route!

Few would dispute another trail leading to the Ukraine from Troitsk. The problem with Tocharian and other IE movements east of the Caspian Sea has not been with origins in the Ukraine. Rather it has been with theories that bring PIE speaking peoples from the west or the south to the Ukraine. Everyone seems to agree that Tocharian at least, if not the Aryan cluster (Indic and Iranian), passed through the Ukraine or somewhere near it on the way to Turkestan. So, at a minimum, we start no great dispute by suggesting that it is now likely (virtually demonstrable) that some PIE speakers moved from Ukraine to southern Ural steppes to Sinkiang. And it is not hard to add that one section of them was the early Tocharians and that this all happened before 2,000 BC.

The ethnologist in me wishes to add that the Tocharians were important intermediaries in the diffusion of Buddhism from India to China later in their history. And given their time and place and Victor Mair's reports of population change over time, it is not so unlikely that the Tocharians were a major instrument in the diffusion of “horse culture” to the

Altaic-speaking peoples of central Asia (Turkic and Mongol), not to mention the same in the form of chariots and chariotteering to the Chinese. Dan McCall also has ideas on the Chinese connection. Whether or not the Tocharians stimulated the Chinese to invent their writing systems as Victor implies, would be a hot topic for someone to pursue.

Some of David Anthony's work was not seen by me before writing this editorial, so I'll not comment on that. Naturally, we hope that this whole discussion provokes our honored colleague, Karl Menges, to comment on various things. This is all very much within the purview of his expertise.

Meanwhile, back in Ukraine, Jacobs is telling us that the robust folk of 5850 BC are the natives, that they were PIE speakers, that they did not migrate in from elsewhere — be it greater Germany as some think or the Balkans or Anatolia as others think —, that they started farming *before the Danubians* could have stimulated them but that ultimately they did have one connection with the outside. They were linked linguistically and archeologically with the adjacent *Caucasic* people, northwest variety, better known to the world as Circassians and Kabardians. The Caucasus Ridge, the main range of mountains in the northern Caucasus, gets pretty close (on the west) to the Crimea and the last outposts or enclaves of Circassians nowadays are only 150 miles south of the mouth of the Don or more importantly they lie contiguous to the Pontic steppes. A linkage between PIE and PNWC is inherently likely — geographically — and indeed has been all along, from our first discussions in 1986.

Nevertheless one would suppose that Jacobs' most enduring contribution is that the Ukrainians of the Neolithic came out of the Ukrainians of the Mesolithic — the farmers came from the hunter-fishers — and not from elsewhere. This supports David Anthony's earlier point that those Ukrainians who began the PIE "horse culture" had local roots which he figured went straight back to the Paleolithic (Upper). Thus, in a nutshell, PIE in its archeological aspect cannot be derived from a movement of farmers from the Danubian water shed and before that from the Balkans and before that from Anatolia. But the Pontic steppes, that lowland grassland between the Black and Caspian Seas, *might* be an area from which pre-PIE people had come.

Why be threatened by Colarusso's hypothesis?

John Colarusso's contribution to our joint endeavors is marvelous! Even if he is wrong, even if he is all wet, it is a marvelous contribution. To reconstruct PNWC (proto-Northwest Caucasian) is hard enough to do in its own right. The demands on advanced computer equipment (Bomhard's) made by John's paper were almost too much. John's thinking was bold and pioneering, yet steeped in the Indo-European verities as many see them. Yet again, not blinded by said verities, he managed to side step the "Indo-baloney" which I love so much. In these respects, he is to be compared with Dolgopolsky, Starostin, Dybo, Bomhard, or Illič-Svityč themselves. Or perhaps more precisely — Paul Benedict.

This will now be the third major and serious attempt

to show that Indo-European is not the last surviving Neanderthal language or the only linguistic phylum created personally by god herself. Humble, mundane, and inferior though they may be, the kin folk of IE do exist. Of course, there have been many attempts to show that IE languages were related — usually binaristically — to some other group. But, taking them collectively and evaluating them for "major" and "serious," the two previous efforts have involved Indo-Semitic which sometimes also includes Afrasian and what we can clumsily call Indo-Ural-Altaic, which has more recently taken the form of "Eurasian" and "eastern Nostratic." And to a great extent for the past thirty years, the concept of Nostratic has embraced both notions, viz, that IE languages had kinship with Semitic and that IE had kinship with Eurasian languages spoken north and east of the Slavs.

Always PIE has been the *centerpiece* of these efforts. Our self-love and self-absorption would not have it otherwise. I say that, like many Americans, as the descendant of three European ethnicities, not one of which would I reject. (Their behaviors often are disgraceful, tis true. But, hey, look at what our American ancestors did to the autochthones of North America!)

The point here is not political or cultural correctness. It is precisely because Indo-European has been the focus of so many of these efforts that they are *vulnerable* to any new taxonomy which puts IE somewhere else. If you took Eskimoan or Gilyak out of Eurasian or Nostratic, who would care? Who would notice? Yet both of them seem to be taxonomically equal to IE in Eurasian. Take IE out of Nostratic and the critics of Nostratic would fall silent or look bored. Without IE, Nostratic is not interesting any more, is it?

Please note that John Colarusso never denies that IE may be related to some languages other than NWC. He is proceeding *binaristically* in a clear way. His heuristic to begin with was the great typological similarity between the two phyla, a similarity proposed by others. The questions that John did not ask are the next important ones. The answers to them will say a lot about how we perceive the situation in western Eurasia when the dust settles. Some questions are:

- What else is NWC related to?
- Is it related to Northeast Caucasian too?
- Is NWC related to Kartvelian (which would then revert to being South Caucasian)?
- What else is IE related to?
- Is IE related to Uralic too?
- Is IE related to both Uralic and NWC?
- Do IE and NWC form a valid taxon over against Uralic, i.e., are IE and NWC closer to each other than either is to Uralic? Or are IE and Uralic closer to each other than either is to NWC? (Probably nobody thinks NWC and Uralic are closer...)

Perhaps those who have not been bored to sleep by the questions can see that there is a collective point to them. Here we are back to the same kind of topic we've encountered before. Is Japanese related to both Altaic and Austro-Tai? Is Sumerian both a Nostratic and a Dene-Caucasian language? Is

Elamitic related to Dravidian more or Afrasian more? Is Italic closer to Celtic than anything else, i.e., does IE include a valid taxon Italo-Celtic? I propose we call this THE GREAT EURASIAN SUB-GROUPING PROBLEM.

If the Nostraticists have done their homework, they should not feel threatened by Colarusso's bold proposal. Is he likely to overthrow all those etymologies, both lexical and morphological? Probably not. If there is real substance to Nostratic — and of course there is —, it will not be overthrown by an extension. Would it be overturned if Samoyedic is found to be related to Yeneseian? (Bouda linked them.) No, because Samoyedic has to be closer — yes, obviously closer — to Finno-Ugrian than it is to Yeneseian.

By the same token, advocates for Dene-Caucasic need not worry that John will show that NWC is closer to IE than it is to NEC (North East Caucasian). My hunch is that John just forgot to mention that “well, yes, of course NWC is related to NEC.” But for the connections between the two Caucasics and Sino-Tibetan and Na-Dene, they do have some grounds for worry. But still, they should not feel too *threatened* because there is also real substance to the Dene-Caucasic hypothesis. It does have a great and a small problem. The small one is that too many of the entities have unreliable or untested proto-languages and too much reliance is placed on the old unreliaables. It's like walking down the street with a broken leg. But one *can* relate languages without reconstructed forms. The great problem is the vast extent of the full Dene-Caucasic hypothesis — from western Europe all the way across Asia to southern Alaska. Too much geography and perhaps too little culture history to associate with it. It does not on the face of it seem to make much prehistoric sense. But it is probably quite a bit older than Nostratic which might account for its problems.

Have Shafer, Sapir, Starostin, Nikolaev, Bengtson, and others produced a considerable amount of evidence for the Dene-Caucasic hypothesis? Yes, in my opinion. Is Dene-Caucasic a valid taxon? For example, might some of its membership actually be closer to some other non-Dene-Caucasic phyla or might Dene-Caucasic as a whole represent more than one large taxon? Well, hmm. And another, hmmm. We really do not know the answer to that.

Why some of us are excited, not threatened, by Colarusso's hypothesis is because of *hope*. The first hope is that his theory, with him being an accredited Indo-European type scholar and all, will break the log jam of Indo-balance which prevents IE from being related to anything else. The second hope is that by presenting a credible alternative to Nostratic or Dene-Caucasic, his “Pontic” hypothesis will force scholars to take the whole topic to a higher level. Relate a lot more phyla to each other and then concentrate on the sub-grouping problem to get a general taxonomy of Eurasia which will do justice to the linguistic complexity there.

Naturally, I mean the *Borean level*. What else? We ran the Borean hypothesis up the flagpole in MT-14 but only one person saluted. It was meant to stimulate discussion but instead it was too rich for most long rangers to even contemplate. But it is time now — if we are ever to get any closer to our goal. (For those not yet fully tuned in, that goal was *Mother Tongue*).

What to make of the Nostraticists at Michigan? Thanks to Irén Hegedűs, we have a report on the so-called “Nostraticist” meeting at Eastern Michigan. We cannot figure out who made out the guest list or what kind of people they were looking for. Lots of long rangers were not invited, even those who are prominent in Nostratic-istics. Feelings which need not have been hurt were hurt. A considerable number of nincompoops showed up and brayed. Perhaps that was all to the good because some of them seemed to moderate their positions.

What was most striking to me was the extent to which the discussion was Indo-European-ized. Apparently they came to focus strongly on phonetic reconstruction but not so much on the substance of Nostratic theory. So no matter what advances are created by our active thinkers, everything will be put to the test of phonetic reconstruction. Only when these self-appointed experts on the mysteries of reconstruction approve of a hypothesis can it be socially accepted. Is that all there is to classification and prehistory? Come on, fellas! Get real!

Long range theory / theory of prehistory: dating and locating or where? and when?

In the last issue of *Mother Tongue*, some ways of looking at homelands were sketched out by Václav Blažek. The sketchiness was due to a lack of translation from the original Czech. Also, more recently, a colleague has said privately that he and some others did not see what was so important about *dating* linguistic groups. Who cares how old Amerind is? Who cares how old PIE is? Those dates are irrelevant to our primary linguistic task — establishing taxa.

So we need to discuss the general theory, or strategy would be a better word, of “where?” and “when?” This “theory” is not empirical, not directly addressed to a body of data which it seeks to explain. Like Dyen's famous “dispersal theory” of some years ago, it is concerned with guidelines to thinking about things so that one can propose better hypotheses. Dyen stressed parsimony, tidy thinking, no unnecessary assumptions, but above all he stressed *taxonomy* overall and *sub-grouping* internally. His critics mostly misread him on this point, thinking that he was just another anal retentive stressing tight control.

In passing, we should note that “guideline” type theory can also be falsified. If one finds a situation where dispersal theory is not useful, then one has falsified it; its advice is not good in that particular situation. Hence no longer is it universally valid. I found two such scenes in eastern Africa. The one in the southern Sudan where a circular arrangement of North Nilotic languages plus rough phyletic equality of the “twigs” led to an impasse. Dispersal theory failed. The other was in Khoisan, where the intrusion of Bantu was to be used as the explanation of the separation of Sandawe, Hadza and the South African Khoisan languages. Dispersal theory failed because the two events had obviously occurred in vastly different epochs.

We cannot finish such an important matter in an editorial. But some rough sketching can be done, even if that is totally contrary to the technical and highly precise spirit of contemporary linguistics. Let us think like astronomers — at

least for a while. Or paleoanthropologists. A pox on all chairpersons, deans, and tenure committees!

How do you find out the location in time and space of proto-Penutian? Well, the best way is to climb into a time machine and have yourself sent back there. When you arrive, you ask the folk where you are and what time it is. Then of course, you record as much of the language as you can while taking samples of their blood and inspecting their artifacts.

Ah, but the technicians cannot send you back to proto-Penutian if they lack the "coordinates." "Give us the date, please, and the map features or coordinates." They might say to you. Even though the time machines of science fiction are magical devices, they try to imitate physics. Vaporous entities like proto-languages lack the necessary physical features; they are not programmed into the magical instrumentation.

In order to use some kind of time machine, we will have to figure out the coordinates of the various languages and/or cultures of the past that we want to visit. Then comes the terrible news that all the time machines are broken and cannot be fixed. We will have to journey back in time *in our minds*. And with some luck, we may find tantalizing traces of the imagined past in our own present — in the archeology of our day. With great luck, we may stand in the ruins of a place like ancient Egypt and look at the pictures they drew of themselves and ponder over the messages they wrote to us in their own languages.

Now there is one other matter — *testability*. Our imaginations are the driving force behind our prehistory, our attempts to find some kind of time machines. Oh, no, you may say; don't be so unscientific. It is our solid methods and technical expertise which generate prehistoric knowledge! Well, they do help a lot, but ultimately, even as in physics where they have far more methods and technology, hypotheses shape the data into intelligible form and appropriate language so we can know the past. Mathematics is a great help too or it can be; it is meant to be included in the sentence above. Nevertheless, we still have to test our theories against some kind of *empirical data*. I understand that many scholars have criticized transformational grammar on precisely these grounds; TG theory is hard to test. Compared to the Holy of Holies, that is a minor matter, chicken feed. PIE itself cannot be tested, at least not directly.

So, therefore, PIE is an unscientific proposition? Therefore, it is nonsense (non-sense, unempirical)? Well, we all certainly hope that is not true! But what does seem to be true is that we cannot go back in a time machine and test PIE directly, by listening to it and recording it. (I'll bet it had no ejectives.) When we decide on which of the many varieties of PIE we will choose, and which of the several sub-grouping schemes we will use, we realize that we have only one way to judge their truth value. That is *indirectly*. Just as TG theory gets tested indirectly by its secondary statements about child learning and how the mind works, PIE has to be tested against the things it predicts, or rather postdicts. Ultimately, that seems to be a test of *internal consistency* or judgments of how well it works to explain its primary data. Goodness of fit. Or something like that (be tolerant, I'm doing the best I can!). But this is all fairly hard to explain to another scientist, especially after all the bragging historical linguists have been doing about

PIE and its wonders as a scientific hypothesis.

There is another way of indirect testing of the PIE hypothesis. The whole concept of IE and its daughters has multiple corollaries and consequences in prehistory and history. Once upon a time, there existed a language with certain phonological properties, certain grammatical features (typological + grammemes), and certain words (morphemes or words whose semantic content was not primarily grammatical). While there is not much one can do to test the grammatical and phonological properties, the lexicon is different. It is at the interface between the core of the language and the culture of its speakers plus the environment they lived in. The concepts of the "semantic component," embodied in the phonology (or incarnate in the sound system) as they were, went a long way towards describing the world the people lived in. There was a word for a kind of tree, a kind of animal, and a kind of spiritual being. And there were real trees and animals and spirits to correspond to the words. So one puts the concepts together and they add up to the picture of some place on earth where the people must have lived, along with the trees and such.

This is, of course, old hat to Indo-Europeanists. From Paul Thieme to Paul Friedrich, and before and after, they have used this forensic technique to locate PIE. Strange that they often differ considerably in their conclusions. Over the years, I have heard of Swedish linguists proposing a PIE homeland in Sweden, German linguists proposing Germany, and Indian linguists proposing (you guessed it!) India. Fascinating, this patriotism! Archeologists often rely entirely on this model for locating homelands and dispersal areas. They get misled a lot.

What good is it to know *only* that the proto-language talked about a land with X type flora and Y type fauna? Archeologically, we know that lands change from time to time, due to climatic change. What had a tundra climate at one time had a taiga at another and a deciduous tree temperate climate at another. No one would propose many parts of the Sahara as a homeland for anyone nowadays because it is too dry and hot. Yet the Sahara has had cooler and wetter periods. No one would locate a homeland under water, like in the South China Sea, but at some periods in the past much of the South China Sea was dry land and tropical forest.

It dawns on us that we must know *how old* the proto-language is, too, in order to locate it via its environmental map. Before we talk about dating, we should note that there are other ways of locating a homeland. One means could be called "historical accidents," such as locating language X near language Y because they have exchanged some words or influenced each other. Thus western Uralic in relation to both the Germanic and the Indic branches of IE teaches us much about each of their earlier locations.

Another means is via so-called dispersal theory. Its greatest use and greatest failure was in the case of PAN (proto-Austronesian). It failed there, not because it was fundamentally mistaken in general, but because it had the wrong sub-grouping. Dyen located PAN in Melanesia because he believed that his lexicostatistical analyses showed the Melanesian branches were the most divergent and therefore the homeland must be in the center of diversity — Melanesia. Yet his own data at the time — which I remember looking at — showed that either Melanesia or *Formosa* had the lowest amounts of shared

vocabulary. He could have chosen Formosa or he could have declared a stand-off between Melanesia and Formosa. Now we know that the greatest number of distinct branches of Austronesian is on Formosa and that the PAN homeland was surely on or near Formosa. Moreover, the fact that Dyen did not accept the genetic connection between Austronesian and Thai-Kadai prevented him from solving a stand-off between Formosa and Melanesia.

Dispersal theory stresses internal taxonomy of a phylum. The more major the branch, the more "weight" it has in relation to the homeland. The less its phyletic importance, the less weight it has in locating the homeland, no matter how much territory it occupies nowadays. Neither the Bantu language group nor Arabic nor English, spread over vast territories though they may be, can outweigh the bulk of their kindred languages. Tiny Frisian clinging to the margins of north Holland equals mighty English spread over half the world. With Dutch added to it, Frisian and Dutch outweigh English as evidence for the west Germanic homeland. But either Greek or Albanian outweighs English, Dutch and Frisian together. And Hittite might weigh as much as all the other IE languages. There are many other examples.

Yet another tactic, used in conjunction with sub-grouping, is unlabeled. It might be called *reason with a map* or *peeling off the superstrata* because it involves taking the whole range of a phylum and slowly evaluating the locations of each twig and each branch until inductively you are left with one unoccupied place to put the homeland. J. P. Mallory used it in figuring out PIE's homeland. It can be used on a region instead of one phylum. It seems greatly superior to other means primarily because it takes into account the other language phyla in one's own territory, as well as any known history. It is empirical or factual, complex, very careful, and possibly the best technique there is. However, it does seem to have a major disadvantage; it is unprincipled (in the Chomskyite usage of that word). When it finds two unoccupied areas, it has trouble choosing between them. Just that happened to Mallory when he came to the Germanic and Slavic areas of IE or simply the north European plain; he had trouble after that. However, the application of "peeling off the superstrata" to Anatolia quickly reveals that PIE is a most unlikely occupant of Anatolia, particularly eastern Anatolia. What gets peeled off in Anatolia (below modern Turkish) is several layers of IE all the way down to Hittite and its tribe, revealing the proper natives to be of Caucasian persuasion underneath it all.

In Europe, where most of us are agreed that a population of farmers moved out of Anatolia up into the Balkans and eventually took over most of Europe and where many of us are agreed that a population of horsemen moved in from the east and took over most of Europe, we can clearly see the need for dating things. Was one movement associated with PIE and the other not? Renfrew and others have proposed a PIE which is associated with the farmers; it is several thousand years older than the PIE others associate with the horsemen. Regardless of the merits of the homelands proposed, Anatolia versus Ukraine, the dates can make a great difference. Were the Celts the first modern people to settle Ireland and are they the same as the first Neolithic folk in Britain? Or did the Celts come in much later than the first farmers?

How does one determine the age of a linguistic taxon? By associating it with a dated archeological culture? That is what most prehistorians seem to do. But that association is often unconvincing and most importantly deprives the reconstructed proto-language of its *indirect testability*. (More on that in a moment.) How do you determine the age of a linguistic taxon by *linguistic means*? Yes, one can use historical accidents again, if you can find them. More promising is the *relative dating* which one can get quite handily via lexicostatistics. Even if absolute dating is impossible, relative percentages do tend to correlate. Lesser percentages suggest "older" while greater percentages suggest "younger." Standard English and standard German relate to each other at about the 70% level of lexical sharing on a Swadesh list, while Latvian and Lithuanian relate at a significantly lower level. Therefore, proto-West Germanic is probably younger than proto-Baltic. On another hand, proto-Australian must be "awfully old" and so too proto-Amerind because between their branches lexical retention gets way down close to zero. Take a look at Fitzgerald and O'Grady's word lists, remembering as you are bound to that they think "word taboo" is responsible for the low percentages.

We would probably have a reasonably reliable absolute dating method called "glottochronology," had the linguists of the 1950s and 1960s not decided to stomp it out. The kinks or problems probably could have been worked out by now. There was nothing inherently unlikely about it, and indeed Dyen and his associates did much of the repair work and had produced a fairly decent revised glottochronology by the mid-1970s. But nobody save a few archeologists will use it anymore. Another item on the taboo list of intolerant linguistics. Is it known that people started "shouting down" Swadesh's new dating method the very day he announced it at a conference? Some say it was because Morris was a Communist, others think he was "just a terrible pain in the ass," still others think linguists simply dislike statistics.

So it seems like a good time to bring it back, this glottochronology, this superbly useful tactic of prehistory. In the next issue of *Mother Tongue*, we will present some of Dyen's "new" 20 year old formulations. It should prove interesting. We might have used Starostin's allegedly new glottochronology but we never solved the communication problem we have with him. So we have stopped trying. Maybe in another life

In sum, then, location and dating are crucial to the most valuable method of testing proto-language hypotheses. Short of climbing into time machines (or telephone booths) so as to hear the proto-language directly, we can reconstruct a *whole prehistoric package* of the following shape:

In time period X in location Y, there lived a people with a culture W and "technoeconomy" T along with flora of type V and fauna of type Q. The people were probably phenotypically of type Z and might show genotypes S if tissue or blood could be used.

Since the package is a proto-language and its accouterments, then there would necessarily be daughters. They both can be tested against the archeology of some region. (To be continued...)

ASLIP BUSINESS

Death of Sherwin Feinhandler

One of the Directors of ASLIP — Sherwin J. Feinhandler of Cambridge, Massachusetts — died on Wednesday, January 5, 1994. Although he had battled leukemia for all of 1993, in 1994, his body succumbed to the infection which his immune system could no longer fend off. Sherwin was a founding member of the Long Range Comparison Club, which became ASLIP; he had been on the Board of Directors since its beginnings.

Anthropologist, psychologist, and linguist though he was, his passion for music ran deeper and more steadily than all his other interests and skills. He was a very interesting man and one many of us will sorely miss. He was one of our very closest friends. But on to business...

Sherwin's obituary is promised for the Spring 1994 issue of *Mother Tongue*. His passing also leaves the Board of Directors rather understaffed. We are very fortunate that Phillip Lieberman of Brown University has agreed to run for election to the Board of Directors in April. Just purely from a numbers standpoint, we would welcome other volunteers to nominate themselves to the Board. One is certainly not required to be a linguist or a lumper linguist to be on the Board. Sherwin would second that notion, since he was in spirit a splitter in some things and a lumper in others.

New Address

ASLIP's formal address is now changed to a new (temporary) address, as follows:

A.S.L.I.P.
c/o Allan R. Bomhard
SIGNUM Desktop Publishing
P.O. Box 6398
Boston, MA 02114-0017

Thanks to Allan Bomhard for lending us this temporary address. You can also correspond with Allan at this address.

With luck, we will have a new permanent address in Massachusetts sometime in 1994, basically after the Fleming family sells its house in Pittsburgh and moves back home.

Annual Meeting

The Annual Meeting of the Association for the Study of Language in Prehistory will be held in Boston, Massachusetts, on April 15, 1994, plus or minus a day or two. Some flexibility in scheduling is necessary for arrangements. A final schedule will be arrived at later, based on the principle that convenience to all should be maximized. The meeting will be held either at Boston University or at the University of Massachusetts, depending upon facilities needed and so forth. Most importantly, however, this depends upon how many

respond to our Call for Papers.

This is also a formal announcement that the annual meeting of the Board of Directors of ASLIP, as well as the election of Officers of ASLIP will be held contiguously.

Call for Papers. For the first time, the annual meeting will feature papers given by long rangers on various topics. the amount of time devoted to papers and the amount of time per paper will depend on the number given. While we do not need to tell people what the range of topics covered is, we do insist that the papers be relevant to our characteristic topics, or pertinent to our pursuits. Papers may be short or long — well, within reason!

Proposals for papers can be sent to any ASLIP officer. The deadline for sending in proposals is April 2, 1994, chosen so that local arrangements may reflect the number of papers.

Publication will be quick and in *Mother Tongue*, unless the volume is so heavy that other arrangements have to be made.

Come on, colleagues! We want to meet you and hear your ideas at length. For non-North Americans, if you are within striking distance of Boston because of some other business, let us know by April 2nd, and we will *try* to schedule things to accommodate your itinerary.

Non-communicative Members of the Council of Fellows

On the question raised in MT-20 about the Fellows who never communicated, there was one small misunderstanding — it was not a Director who was the guilty party. Had it been, the Board of Directors is amply empowered by our By-Laws to replace one of its members. However, when the question involved a member of the Council of Fellows, elected by the membership at large, it became quite another matter. Therefore, we thought it advisable to solicit the opinions of our full membership and then have the matter decided either by those in attendance at the Annual Meeting or by the Board of Directors.

Since then, one of the non-communicative Fellows has paid both back and current annual dues. We reckoned that this was communication enough. The remaining Fellow, however, continues to be unresponsive.

What to do? Three members have expressed opinions. One argued that the Fellow was probably ready for retirement and had other things to think about — so be nice. The second said that we should not keep honoring someone who was so disdainful of our collectivity. The third recommended that we throw the rascal out, saying, among other things:

When you are a member of an organization (especially when you are an officer), you have a responsibility to that group and its members. Brilliant or not, their arrogance should be rewarded with ostracism from the linguistic community.

This leads to another clarification. Actually, Fellows are *not* officers of ASLIP, nor do they belong to the Board of

Directors. They have no formal responsibilities except to stay members by paying their annual dues. Our feelings towards our recalcitrant Fellow derive from two facts: (1) The whole membership has honored them, selected them to be outstanding, and caused their names to be put on our masthead;

and (2), after being treated so famously, our hero gives the impression of being disdainful of those who have honored him or perhaps of being so disinterested or so lazy and/or so self-centered that many years pass with nary a word from him about anything.

WORLD ARCHAEOLOGICAL CONGRESS — 3

New Delhi, India, 4-11 December 1994

MAJOR THEME: LANGUAGE, ANTHROPOLOGY, AND ARCHAEOLOGY

Theme Organizers: S. P. Gupta (India), R. M. Blench (England), M. Spriggs (Australia), and C. Renfrew (England)

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Communications (between 30/XII/93 and 18/III/94): Dr. Matthew Spriggs; Department of Prehistory;
Research School of Pacific Studies; Australian National University; Canberra, ACT; Australia
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Please note that paper titles should be sent to the organizers in New Delhi, while both title and a short abstract should be sent to the liaison in the United Kingdom (Roger Blench).

The focus of this theme is the relationship between language and archaeology, very broadly defined. This ranges from the biological (origins of language, genetics, and linguistics) through social and historical (sociolinguistics, oral tradition, etc.) to the wider issues of correlating linguistic hypotheses with archaeological data. It also has a regional sub-theme, although papers in this are intended to illuminate broader methodological issues.

A) RELATING ARCHAEOLOGY AND LANGUAGE: The relationship between “language” and “culture”, the origins and evolution of language, processes of linguistic change, and their archaeological implications. This consists of a series of primarily methodological papers.

1. **Archaeology / Biology and the Origins of Language.** The antiquity of human language remains extremely controversial. Archaeological evidence has been used to date its first appearance, but no one schema has yet gained general acceptance. *Co-ordinators:* Iain Davidson (UNE, Australia) and Andrew Lock (Massey U, New Zealand).
2. **Problems in the Definition of Macro-Phyla and Possible Archaeological Correlates.** How related are the world's languages and how might this have implications for the spread of modern humans? *Co-ordinator:* Colin Renfrew (Cambridge U, UK).
3. **Implications of Human Genetics for Language Grouping.** Recent studies in various areas of the world at macro and micro-level are providing fascinating evidence of human genetic groupings in relation to language boundaries, and bringing out new theories to explain the fit or lack of fit in particular cases. *Co-ordinators:* Rebecca Cann (U of Hawaii), Kenneth Kidd (Yale U, USA), and Susan Serjeantson (ANU, Australia).
4. **Language and Prehistoric and Historic Migrations.** Examines the archaeological evidence adduced for migrations, along with the perhaps cautionary tales of the archaeological evidence (or lack of it) for historically known migrations which have had a linguistic impact. *Co-ordinators:* V. Alekshin (Institute of Archaeology, St. Petersburg, Russia), John Hines (U of Wales, Cardiff), and Kristian Kristiansen (Copenhagen, Denmark).
5. **Dating Language Spread and Change.** Examines the somewhat instinctive feel linguists have for how quickly languages change, hopefully to make more explicit their reasoning and the extent to which it is based on now-perhaps discredited methods such as glottochronology. Attempts to calibrate linguistic change to radiocarbon dates will be considered. *Co-ordinators:* Malcolm Ross and Matthew Spriggs (ANU, Australia).
6. **Language and Society: Variation and Change.** Includes topics such as language diversity, trade languages, pidgins and creoles, language leveling, language switch, and obsolescence. All of these sociolinguistic processes can be expected to have archaeological implications but have been rarely considered by archaeologists. *Co-ordinators:* Tom Dutton, Darrell Tryon (ANU, Australia).

7. **Proto-Lexicons and the Origins of Agriculture.** How far can linguistics be used to reconstruct vocabularies relating to the "homeland" of particular language families, and to the subsistence practices of the speakers of reconstructed proto-languages? Can such reconstructions be correlated with archaeological manifestations relating to the origins and spread of agriculture? *Co-ordinators:* Robert Blust (U of Hawaii) and Peter Bellwood (ANU, Australia).
8. **Geographically-Informative Semantic Fields.** Animal and fish names, flora, and meteorological terms can help place the locations of particular language stages or in showing connections between areas. Toponymy is perhaps an old-fashioned topic in Europe but may be worthy of further consideration. *Co-ordinator:* Jean-Marie Hombert (Université de Lyon, II, France).
9. **Oral Traditions, Myths, and Archaeology.** Considers traditions and myths of origin and other methods of self-perception in relation to archaeology and language. Examples include French work in the Pacific attempting to relate voyaging traditions and historical migrations, and Australian research examining Aboriginal stories in relation to movements of groups and languages. *Co-ordinators:* Daniel Frimigacci (CNRS, France).

B) THE ARCHAEOLOGY OF LANGUAGE REGIONS: A series of case studies bringing in the methodological concerns of earlier sessions and a summing up of the major theme. It will also give the opportunity to present more specialist papers relating to particular language groups.

1. **East Asia.** Co-ordinator: Gina Barnes (Cambridge U, UK).
2. **Europe / Asia.** Co-ordinators: J. P. Mallory (Queen's U of Belfast, Northern Ireland) and Victor Shnirelman (Institute of Ethnology, Moscow, Russia).
3. **Central Asia / Himalayas.** Co-ordinator: George van Driem (U of Leiden, The Netherlands).
4. **Indian Subcontinent.** Co-ordinator: S. P. Gupta (New Delhi, India).
5. **Indian Ocean.** Co-ordinator: Claude Allibert (CEROI-INALCO, France).
6. **Southeast Asia and the Pacific.** Co-ordinator: Andrew Pawley (ANU, Australia).
7. **Australia.** Co-ordinator: To be identified.
8. **Africa.** Co-ordinators: Roger Blench (Cambridge, UK) and Kay Williamson (Port Harcourt, Nigeria).
 Subsession: **Eastern Africa.** Co-ordinator: Mark Horton (Bristol U, UK).
 Subsession: **West Africa.** Co-ordinator: Roger Blench (Cambridge, UK).
 Subsession: **Southern Africa.** Co-ordinator: Rainer Vossen (U Munich, Germany).
9. **The Americas.** Co-ordinator: To be Identified.

The following timings are proposed:

1. **Titles:** As soon as possible. These should be sent to Roger Blench at the above address and to the organizers in New Delhi, along with registration.
2. **Abstracts:** By 1st February 1994. Half a page to Blench but not to New Delhi. Blench will forward them to the co-ordinators of individual sessions.
3. **Complete Papers:** By 30th June 1994. One copy to Blench, one copy to the organizers in New Delhi. For multiplication purposes, the paper should contain no more than 3,500 words, although this is not a restriction for publication purposes.

December 1994 seems far away, but with these schedules, it is not all that far.